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ESSAYS, MONOGRAPHS, AND CASES.

Treatment of Phthisis by the Chlorate of Potash; with Observations on Oxygen and Ozone as Therapeutic Agents. By E. J. Fountain, A.M., M.D., Davenport, Iowa.

"Oxygen is the leaden weight or bent spring which keeps the clock in motion; the inspirations and expirations are the motions of the pendulum which regulate it."—Liebig.

(Read before the American Medical Association, at its Thirteenth Annual Session, held at New Haven, June, 1860.*)

In discussing the properties of the chlorate of potash as a therapeutic agent, I have formerly ventured the prediction, that it would be found a valuable remedy to arrest the development of tubercles, and promote the absorption of those already formed, when not too far advanced.

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* Extract from the Report of the Section on Practical Medicine and Obstetrics.—"A voluntary paper on the Treatment of Phthisis by the Chlorate of Potash, with Observations on Oxygen and Ozone, as Therapeutic Agents, was then read by the author, Dr. Fountain, of Davenport, Iowa. It was listened to with much attention, and after some discussion, it was

In the July number of the *N. Y. Journal of Medicine*, 1859, I called the attention of the profession to the importance of meeting a frequently occurring indication arising in the progress of many diseases from *imperfect aeration of the blood;* and reported cases in illustration of the fact, that in the chlorate of potash we have an agent fully capable of filling this indication. The cases reported were at once demonstrative of its power and reliability in this respect, and also of its great practical utility under circumstances so unfavorable as to preclude all hope of recovery by any other treatment. In addition to this, I endeavored to establish the theory, that in many instances where the blood is not deficient in its usual supply of oxygen, absorption of many organic products may be promoted by rendering the blood more highly arterialized through the agency of the chlorate of potash. Among these, were included tubercular deposits, as these were believed to be the result of an imperfect elimination from the system of the products of organic decay of the tissues of the body, and are composed principally of protein compounds, which are rendered soluble by the addition of one or two equivalents of oxygen, converting them into the deutoxide or tritoxide, and permits their absorption by endosmosis into the adjoining vessels—it was suggested that the chlorate of potash might be found a valuable remedy by supplying the blood with an excess of oxygen sufficient for this purpose. Although the disease until quite recently has been comparatively rare in Iowa, yet I have had the opportunity of testing the theory in the following cases—with how much confirmation the profession must decide.

"*Resolved*, That the Section has listened to the paper with deep interest, and suggest that the paper be referred back to the author, with the request that he pursue his investigations and report to the next meeting of the Association."

This request of the Association is my apology for the publication of the Essay, by which the purpose of the above resolution can be best accomplished.

Whatever may be the result of this method of treatment, the cause of medical science cannot but be advanced by any well-directed investigation relating to such a widely prevalent and fatal disease as Tubercular Consumption. Hoping I may have contributed something to our stock of knowledge on this subject which will lead to practical, good results, and attract the attention of the profession to the investigation of the value of oxygen in its diversified application as a remedial agent, I respectfully submit the following, not as a conclusive demonstration of any theory or practice, but as suggestions which may lead others to test their value and prompt investigation in a new direction. The author will be pleased to receive contributions on the subjects referred to, from any of his professional brethren, that sufficient material may be obtained for a report before the next meeting of the American Medical Association.

E. J. F.

Case I.—Miss B——, a young lady, aged 19, recently from the East; strongly predisposed to phthisis by both parents, each of whom died with this disease while she was very young.

About the first of October, 1859, she had a fall, producing considerable concussion of the body generally, but apparently no serious injury. Symptoms of fever followed in a few days, which were at first thought to be the result of the fall, but soon they were developed into evident indications of autumnal malarious fever, with slight periodic remissions. Treatment appropriate to this condition was adopted, and followed by an arrest of fever, and apparent convalescence. After being up a few days she was again obliged to take her bed by a return of fever of a low continued type, accompanied by a dry cough, which latter symptom soon attracted paramount attention. The fever now lingered uninfluenced by any anti-periodic treatment, the cough in the mean time increasing in severity. At this period, no decided symptoms of tubercles could be detected by auscultation, and yet there was no evidence of bronchitis or pneumonia. I faithfully tried all manner of expectorants and sedatives without benefit. The cough was always aggravated by opiate preparations, and even the remedies to promote expectoration recommended by Dr. Jacobi, oxysulphuret of antimony and hydrochlorate of ammonia, were each tried, with no better result.

Her strength steadily failed, the cough becoming more persistent, and the fever assuming the character of hectic. An appreciable degree of dullness was now recognized under the clavicles on each side—more especially on the left, where the respiratory murmur was less distinct than natural. Knowing how strongly predisposed she was to phthisis, and unable to account for her symptoms and their persistence on any other view, I now became satisfied that tubercles were being rapidly developed. This was about three weeks from the commencement of her sickness. Churchill's remedy of the hypophosphites of lime and soda and cod-liver oil were now administered, and other adjuvants, as symptoms indicated; but the disease progressed rapidly, and without any abatement from this treatment. Marked hectic fever supervened; increased cough; profuse night-sweats; rapid emaciation and loss of strength; and sleepless nights. The rational symptoms were more characteristic and well defined than the physical; but the latter, though somewhat obscure, were sufficiently evident, in connection with the above, to render the nature of the disease unmistakable. The case excited unusual anxiety in my mind, as the patient was a near relative, and, for a time previous to her sickness, a member of my own

family. Every possible effort of my own, aided by the advice of my professional friends, was made for several weeks following; but steadily the disease progressed, and soon was impressed in unmistakable character upon her countenance, and manifested by all her symptoms.

About six weeks from the first attack of her illness, despair of her recovery began to settle upon the minds of her friends and medical attendants, and the patient herself could no longer be buoyed up by any hope. As yet no expectoration had appeared, and it seemed as if the disease would terminate fatally, as it does in some rare cases, from the depressing influence of the tubercles before reaching the stage of suppuration. She had at this time scarcely strength sufficient to change her position in bed without assistance; no inclination for any nourishment; extreme pallor of the skin, changing to a bright hectic flush every afternoon; rapid pulse and respiration; constant and harassing cough; profuse night-sweats; sleepless nights; and extreme prostration. The "fell destroyer" had evidently, to our minds, marked her as its victim; and though anxiously and faithfully applying every approved method of treatment, we were conscious of seeing her yielding rapidly to its power, in defiance of every effort to stay its progress. Finding all treatment utterly futile, it now occurred to me for the first time that I had, on theoretical views, suggested the use of the chlorate of potash in such cases, and I now determined at once to give it a trial. I had a number of packages of the chlorate put up, half an ounce in each. One of these I dissolved in about a pint of water, and directed her to drink it *ad libitum*, so that the whole of it should be taken in twenty-four hours. Then another to be prepared and taken in the same way—half an ounce each day, and no other medicine. The change was like magic, using the expressive language afterwards frequently repeated by the patient. She began the first day to experience decided relief, and in forty-eight hours the cough had so far abated, and the oppression of breathing reduced, that she slept well for the first time for several weeks. The last four nights preceding this treatment had been passed in extreme restlessness—almost constant cough, and without any sleep whatever. The respiration and pulse diminished in frequency, night-sweats and fever disappeared, and her appetite and spirits revived. In four days her cough had almost entirely disappeared, and not a trace of it was left in one week after commencing this treatment. By this time she began to sit up, to eat with a good appetite, and sleep soundly every night. Rapid convalescence followed. In another week she was so far restored that she left her sick-room permanently, and has since remained in the full

enjoyment of health. She took half an ounce of the chlorate of potash every day for nearly two weeks, and gradually reduced the quantity during the week following, when it was discontinued altogether. Not a particle of cough has ever returned, and no symptoms of her recent alarming illness. She has since returned to her Eastern home, in Newark, New Jersey, perfectly restored to health.

Case II.—Mr. M——, aged 25, family consumptive on mother's side, applied to me for treatment, Nov. 5th, 1859. Had been living for about two years in Omaha, Nebraska. Health usually good during a number of years past, until within the three or four months previous to calling upon me. Has been coughing for three months, during which period he has expectorated blood several times. Strength gradually failing, losing flesh, and appetite entirely gone. He assured me that for two months he had not taken nourishment sufficient to support properly the strength of an infant; and his emaciated and bloodless appearance corroborated the statement. He had some soreness in the throat, and complained of pain occasionally on the right side. Cough very frequent night and day, but very seldom any expectoration. His debility was so great that he fainted from the effort of standing erect to permit a physical examination of the chest. The left lung appeared to be perfectly healthy, but there was appreciable dullness over the middle and upper lobes of the right. Over this region of dullness the respiratory murmur was very feeble and indistinct, and the expiratory sound nearly equal in duration. His respiration and pulse were increased in frequency on the slightest exertion, and he complained much of great oppression in breathing. He had not the slightest desire to eat at any time, and seldom more than tasted food.

Treatment.—In this case I decided to test the chlorate of potash alone before giving anything else, that I might not confound the effects of different remedies, and thus be able to determine more accurately the properties of the one under investigation. I therefore prescribed half an ounce to be taken daily in the same manner as in the preceding case. This was on the 5th of November.

Nov. 8th.—Decided improvement; cough less; oppression greatly relieved.

10th.—Still improving, and beginning to have a little appetite.

Satisfied by this time that the chlorate was having a decided effect in the manner I anticipated, I now thought it proper to aid the treatment by prescribing iron and quinine in small doses daily, and continued the chlorate of potash the same, half an ounce each day.

6th.—Cough very slight; no pain in the chest; full inspiration taken with less difficulty; general appearance decidedly better.

19th.—Improvement continues unabated. From this date I reduced the chlorate of potash to three drachms daily.

23d.—Patient considers himself almost well. Appetite good; no cough; gaining strength and flesh quite rapidly; still a slight degree of dullness on the right side; but respiratory murmur more natural. Continue the chlorate of potash in drachm doses three times a day, and omit the iron and quinine.

Soon after this he left on business for the East, and on the 18th of January following, he addressed me a letter relating to his health from Washington City, D. C. He had continued taking the chlorate of potash most of the time. He invariably felt worse by omitting it, and renewed strength and general improvement when he returned to its use. In this letter he says: "I have no cough, but occasionally some difficulty in breathing; but not near so much as when you first saw me at Davenport in the fall. My strength has improved a great deal, and generally my appetite is quite good."

CASE III.—Mr. H——, aged 34, placed himself under my care early in November, 1859. He had the appearance to every one of a man sinking under the influence of confirmed phthisis. To this he was predisposed from his father, who died young with this disease—mother still living and well. A gradually increasing cough and failing health had been gaining upon him for the past five years.

Once during this period he had improved under the use of cod-liver oil and phosphate of iron. Free, and quite profuse, hæmorrhage from the lungs once, and slight traces of it a number of times during the past year. When he applied to me he was conscious of losing strength very fast. Marked emaciation, and unhealthy expression of countenance; very frequent cough, but seldom any expectoration; no appetite; respiration hurried and oppressed; pulse seldom below 90; moderate dullness on the right side, over the infra-clavicular region; no râles, but respiratory murmur indistinct and irregular. As he had once been benefited by cod-liver oil and the phosphate of iron, I first prescribed this same treatment, thinking it might again have a similar effect. This was continued faithfully for about two weeks without any material benefit, when I directed it to be discontinued, and prescribed the chlorate of potash alone, half an ounce daily, as in the above cases. In less than a week he assured me that he felt a decided benefit from the treatment. The improvement continued steadily from this time, and he completely regained his health and

strength in less than three months. *He took half an ounce of the chlorate of potash daily for six weeks*, and two drachms each day for the succeeding four weeks; since which time he has taken it only occasionally, and in smaller quantity. At the present time of writing, (April, 1860,) he is actively engaged in business, in good strength and flesh, having no cough, except a trifle from a recent cold; complexion perfectly healthy, and appetite good. His own feelings and general appearance indicate a perfect restoration of health.

Remarks—Although these cases are not sufficient to verify the prediction which I ventured to make nearly a year ago, they certainly furnish strong presumptive evidence in its support. Of themselves they are sufficiently striking to merit the attention of the profession, irrespective of any particular theory which prompted the treatment. But cases reported can be regarded as contributions to science only as they sustain or reveal some principle of general application. In these cases, although the treatment was purely experimental, it was not empirical; for the chlorate of potash was given on the assumed principle of *conveying oxygen to the blood*, by which I expected to relieve the lungs of a portion of their task; increase the vital power of the blood, and render it more capable of faithfully performing all its functions; and by which tubercular deposits might be arrested, and absorption of those already formed promoted. So far as I have yet been able to test the practice, the results have more than realized my anticipations, and sufficient at least to justify me in laying the subject before the profession. Whatever may be the result of a more enlarged experience, some important facts are established by the foregoing cases, and others which have been elsewhere reported.

1. The chlorate of potash can be given in large doses every day, for a long period, without injury.

2. It aids the functions of respiration, by supplying the blood with oxygen.

3 It operates as a natural *tonic*, *alterative*, and *blood depurant*, by increasing the supply of that element which is the most active agent of nature in the chemical changes which take place in the laboratory of the human system.

I will now add, that in the practical application of these principles, it is my belief, which may or may not be confirmed by the experience of others, that it is a peculiarly appropriate remedy for the *early stages of phthisis*, by which the resolution and absorption of incipient tubercles may be effected, and their further deposit arrested. Even where the disease has progressed to the second stage, I have found

patients derive great benefit from the use of this remedy; more, indeed, than from all others, single or combined. I cannot, however, expect it to be of permanent benefit, only in the early stages of the disease, before there is any disorganization of the lungs or suppuration of the tubercles. To all who are laboring under the symptoms of this disease, before it has progressed to such an extent, I would recommend the liberal and constant use of the chlorate of potash, aided by appropriate hygienic treatment, in which exercise and pure air hold the rank of first importance. Half an ounce daily is the quantity I usually give, when I wish to produce a decided effect for any purpose, and this can be taken certainly for many weeks with impunity; but when I have occasion to give it for a long time, I find three drachms a day can be depended upon as sufficient in most cases. After all symptoms of phthisis have been arrested by this treatment, it should still be continued for a time in moderate doses, and always resumed as soon as the slightest symptom of its return is manifested. In addition to this treatment, I would recommend as a daily beverage a trial of liquids artificially charged with oxygen gas.

If it should be asked why not give the oxygen directly by inhalation, the answer is, that too much irritation of the bronchial mucous membrane will be produced by any excess of the natural proportion of oxygen in the air that is breathed; and further, that in phthisis and other pulmonary affections, it is desirable to *relieve* the lungs of a portion of their duty—not to increase it by over-stimulation, and imposing an additional burden. Moreover, the inhalation of oxygen has been thoroughly tested long ago, and abandoned as useless. The following is from a paper of Dr. H. Bence Jones, F.R.S., &c., published in Part XXV. of *Braithwaite's Retrospect:*

"No sooner was oxygen discovered, than an attempt was made to apply it as a medicine. The Pneumatic Institution, as it was called, was founded by Dr. Beddoes, for the purpose of using oxygen as a remedial agent for all kinds of disease; but though it was supported by the energy and truthfulness of Dr. Beddoes, and by the sense and skill of James Watt, the great engineer, who applied himself with the greatest zeal to the carrying out of the mechanical process of administering the oxygen to the patients; and though it called forth the talent of that great chemist to whom we are so largely indebted—Sir Humphrey Davy—yet it utterly failed in accomplishing the object for which it was established. No satisfactory conclusions were arrived at; no power was obtained of using the oxygen as a remedy, and the undertaking came to naught, not because the oxygen was impo-

tent, (for it is the most powerful agent in nature,) but because the means of applying it advantageously were unknown; and they remain equally unknown now. We know no more now respecting the means of applying it as a remedy, than was known in the time of Dr. Beddoes and Sir Humphrey Davy. At present all attempts to employ it as a remedy must be described as having altogether failed."

Thus it will be seen that oxygen has been thoroughly tested for remedial purposes by men of the highest scientific attainments, and abandoned as impracticable, because no means of applying it advantageously could be discovered; and I think it is conclusive, that by no method of inhalation can it be administered with benefit. The observation of Dr. Jones, that the means of applying it advantageously "remain equally unknown now," undoubtedly expressed the truth at the time it was made, in 1852; but it would not be true if made at the present day.

I think I have demonstrated in the paper before referred to, that oxygen can be administered in the form of a chemical combination for the purpose of aiding the operations of nature in arresting disease; and evidence has been furnished of its powerful influence in this direction, proportionate to its potency in the world at large; and may yet be found sufficient to realize the sanguine hopes of Beddoes, Watt, and Sir Humphrey Davy. The genius of man has made the lightning of heaven subservient to his will, as he makes it the swift messenger of his thoughts; and strange would it be if an agent so powerful and so full of life-sustaining properties as oxygen should not be brought into subjection to antagonize the effects of disease.

Of late, we see reports of *ozonized oil* being administered with benefit in phthisis. In my opinion, it is no more nor less than so much oxygen administered with the oil. The oil is charged with oxygen, and exposed to sunlight, and then it is called ozonized oil. This is an arbitrary assumption, and not the deduction of close analytical reasoning or experiment. There is a cloud of mystery connected with the subject of ozone which modern science has not yet removed by reports heretofore made on the subject.

My attention has been directed to this subject in connection with recent investigations in relation to oxygen as a therapeutic agent. My observations have led to the development of a theory of the nature of ozone, which I believe to be more in accordance with well-known laws in natural philosophy than any heretofore advocated; and while it embraces, as I believe, all the facts which have been observed in relation to the subject, it clears up many of the difficulties which

have been acknowledged to belong to it. Its practical bearing upon the subject under consideration will be apparent from the sequel.

What is ozone? To this query scientific men have given different answers, and still entertain a discrepancy of opinion.

Schönbein has regarded it as a peroxide of hydrogen, but more recently he maintains that "there are two kinds or allotropic modifications of active oxygen, attending to each other in the relation of + to —; *i. e.*, that there is a positively active and a negatively active oxygen—an ozone and an ant-ozone, which, on being brought together, neutralize each other into common or inactive oxygen."—*Amer. Jour. of Science and Arts*, vol. 27, p. 19.

Dr. Pickford calls it a teroxide of hydrogen.

De la Rive and *Berzelius* consider it to be nothing but allotropized oxygen.

Dr. Prout regards it a deutoxide of hydrogen, formed by the excess of the twenty per cent. of oxygen, which, he says, there ought to be in the atmosphere, associated with the vapor of the atmosphere.

Scoutetten, and many others, believe it to be electrified oxygen; and by others, still, it is regarded as simply oxygen thrown into a state of activity by electricity.

Dr. James H. Pickford, in his recent and most excellent work on Hygiene, advocates the same theory as Dr. Prout, namely, that the excess of oxygen in the atmosphere, at particular times and places, is converted into ozone, by reason of this very excess. Why this should be the case, he does not pretend to explain. The opinion cannot be sustained by sound philosophy, and it is surprising that it should be advocated by Dr. Pickford, when, in his same work, he has stated facts which prove its fallacy. According to this theory, the relative proportion of oxygen in the atmosphere must be constantly varying, and upon this variation must depend the presence or absence of ozone; and none can exist where there is not an excess above what is assumed to be the exact normal proportion of twenty per cent. Let this theory, or hypothesis, rather, be compared with the following passages from Dr. Pickford's work.

On page 118, he says: "By the reciprocal action, therefore, of plants and animals, the composition of the atmosphere is preserved *nearly absolutely unchanged*." (The italics are thus made by the author of the language quoted.) This excess of oxygen he attributes to the action of vegetation exhaling it into the atmosphere; and yet, on page 118, he again says: "Supposing the earth to be peopled by 1,000,000,000 men, and its animals to be equivalent to 3,000,000,000

more men, and plants to have *ceased from their functions* over the entire surface of the earth, it may be shown that the oxygen of the atmosphere would not be diminished to a greater extent than $\frac{1}{80000}$th part of its entire weight during a century; and further, that no sensible effect would be produced on Volta's endiometre in a less period than 10,000 years."

These facts need no comment. They are certainly sufficient to demonstrate that ozone cannot be occasioned by any varying excess of oxygen in the atmosphere. Moreover, were this a true theory, ozone should be found always more abundant within and over dense forests than other places, which is not the fact; and over the ocean and the barren sea-shore, where it is most abundant, there should be none at all. In the face of facts like these, it is indeed surprising that such an opinion should be advocated by Dr. Pickford and Dr. Prout. I think I can make it apparent that the views of Scoutetten, Schönbein, De la Rive, and others, are also incorrect, and unequal to a satisfactory explanation of all the phenomena of ozone.

By these writers and the scientific world generally, it has been supposed to be a definite substance in some shape mingled with and floating in the atmosphere. This, I think, is not the fact. On none of these theories can any satisfactory explanation be made of at least two facts which I have observed.

1st. Ozone is not found within doors except in very slight traces, and generally not at all, however well ventilated the building may be; and

2d. It is not found under the deep shade of the forest. The observations of Dr. J. H. Rauch first called my attention to this latter fact.

During the warm months of last summer the ozonometer always indicated an abundance of ozone in the atmosphere immediately without the open window of my library, while within the same scarcely a trace of it could ever be observed, even when the windows were open and pure air circulating freely in the room. If it is something floating in the atmosphere, and forming a part of it, why will it not enter within a dwelling? and why does it not circulate with the air in the shade of the forest? and why, let me also ask, will it not circulate through an atmosphere which is perfectly dry? If no satisfactory answer can be given to these queries on any theory which has been mentioned, I think a rational explanation can be found in another which I am about to offer.

I shall venture to affirm that *ozone is simply oxygen in a nascent condition, produced naturally by electrolytic decomposition of the vapor of*

water in the atmosphere. It is a law in chemistry that all gases, when in a *nascent* condition, possess properties and powers which are peculiar to that state. In this manner gold is dissolved by the nascent chlorine generated in the *aqua regia;* and the bleaching power of chlorine is due to its strong affinity for hydrogen, by which the water of the dampened fabrics is decomposed, and the oxygen liberated; and this nascent oxygen, or ozone, is the true and proximate bleaching agent. As a disinfecting agent it probably acts in the same way, by decomposing the vapor in the atmosphere and liberating the oxygen.

Electrical currents are constantly and silently passing around the earth, following the direction of the sun, and upon the combined effect of the degree of intensity of these currents, the discharges of electricity from the clouds, and the amount of vapor, depends the varying proportion of ozone in the atmosphere.

It is not oxygen electrified or hydrated, but oxygen generated from the decomposition of water or the vapor of water; and it is ozone only at the time of this decomposition. It is not a thing of itself, which can travel with the air and be separated from it, but a manifestation of peculiar properties of oxygen when in a certain condition, which may be termed *dynamic;* and these properties can be manifested only at the time and place of the liberation of oxygen from some chemical combination. This will explain why it is that ozone cannot be produced in an atmosphere which is *perfectly dry*, and also why it is not found within a dwelling or under the shade of the forest. The currents of electricity at all times flowing through the atmosphere are interrupted and pass to the earth through the medium of any prominent structure or forest-tree. They pass down through the walls of our dwellings and circulate not in the atmosphere of our rooms, and therefore we find no ozone there; and the trees of the forest also act as conductors to these electrical currents, and thus prevent the development of ozone beneath them.

In the laboratory, by means of phosphorus, the phenomena of ozone can be made to appear without the aid of electricity, *but never without water*. And so in the atmosphere, ozone can never be produced by natural agencies without the presence of the vapor of water.

In the proceedings of the American Association for the Advancement of Science, held at Montreal, in 1857, Prof. Charles Smallwood says, that from his observations it would appear "that a *moist* and *humid* atmosphere was necessary for the development of ozone, and this may account in some measure for its more constant presence and its greater quantity in proximity to the sea."

In the Smithsonian Report of 1857, page 390, I find the following:

"In the electrolytic decomposition of water, the presence of ozone is manifested at the positive pole, *where the oxygen is given off*."

This confirms the theory now offered. We there have a most perfect exhibition of oxygen in a *nascent* condition, and consequently a constant manifestation of the phenomena of ozone. I will venture to say that the same phenomena will always be found in the presence of oxygen which is being generated from the chlorate of potash, or any other chemical combination.

On page 391 of the last-mentioned Report the writer says: "Electricity prepares the vapor of the atmosphere to oxydize further and form ozone; in like manner phosphorus effects the combination of the vapor of water with oxygen; but, as yet, we are not able to tell *how* it is done." May not this mystery be removed by referring all the phenomena of ozone to *nascent oxygen*?

In this experiment, it will be observed that ozone is produced without the aid of electricity, but the presence of water is essential. The same writer, Dr. Müller, of the University of Freiburg, says: "In perfectly dry air ozone cannot be obtained by means of phosphorus." He takes it for granted that ozone must be a compound of oxygen and water; but *how* such a combination is effected, and *why* it should manifest such powerful oxydizing properties, he is unable to determine; and these are the very difficulties which are avoided by the theory which I have offered. There is another difficulty in the way of believing it to be a compound substance of any kind. The great and peculiar characteristic of ozone is its powerful oxydizing property; and if it is a compound of oxygen and water, it cannot operate in this manner without itself undergoing decomposition; and this will be as difficult to explain as its first formation from the same elements. If oxygen, by the influence exerted in some unknown way by electricity, or phosphorus, is made to have such a strong affinity for water that it unites with it in the form of a chemical combination, what power compels it to forsake this alliance and fly so quickly to manifest this same remarkable affinity for other substances, with which it has no power to unite at other times? But where, it may be asked, is the nascent oxygen, in the experiment of generating ozone by phosphorus? Can we not find it in the supposition that the phosphorus decomposes the water, as we know it is capable of doing, under certain circumstances, forming phosphuretted hydrogen, and liberating the oxygen in a nascent state? And why may not this process go on for a limited time in a bottle containing air charged with the vapor of phos-

phorus and the vapor of water? Careful experiments, such as I have not had the opportunity of making, are required to determine this point to a certainty; but theoretically there is nothing in the way of believing this to be the truth, and there is much to substantiate it by analogy, while it certainly does less violence to our reason than the adoption of a theory for which it is admitted *no* explanation can be given. The analogy is found in the fact that, in order to develop the phenomena of ozone by any of the natural or artificial methods yet known, aside from this, two conditions are required: namely, the presence of water or its vapor, and something which is capable of decomposing water and liberating oxygen, as *electricity* or *chlorine*. When we reflect upon the known properties of nascent oxygen, and consider that its presence can always be accounted for, whenever and wherever ozone is found, the inference is almost irresistible that the two are identical—in truth, one and the same thing; and especially is it rational to admit this as a scientific fact, if by so doing we can resolve the subject under the operation of a simple and well-established law in chemistry, and divest it of all the mystery which has so long baffled investigation.

By this theory, we can understand why and how the presence of ozone can be made to appear through the agency of phosphorus, chlorine and electricity, in connection with water or the vapor of water. And what, let me ask, is there in common with these three totally distinct agents? One is an element existing in either a solid or gaseous state; the other is an element very different in character, and never solidified, while the third is altogether immaterial and imponderable; and yet each has the one, and only one common property of decomposing water and setting its oxygen free; (at least the fact will be admitted in reference to the two latter, and I believe it will be found to be true of the former.) Connect this with another fact, that ozone is produced by each of them only when water is present, and it appears to me the inference must be inevitable, that under all circumstances, ozone is nothing more nor less than nascent oxygen.

"The earth," says Dr. Pickford, "may be viewed as one huge electrical machine, of which the direction of the currents is from east to west." And again, he says, "The atmosphere abounds with electricity, which it derives from the thermo-electric agency of the earth." Why may not these ever-prevailing electrical currents and the more powerful discharges from the clouds be capable of decomposing the vapor of the atmosphere, as well as water itself is decomposed by the currents proceeding from the poles of a galvanic battery? Negative

and positive electricity in the atmosphere can operate in the same way as the negative and positive poles of a battery. Admitting the possibility of this, which can hardly be denied, and the solution of all the mystery of ozone becomes simple and satisfactory; and *why* it possesses such remarkable oxydizing properties is fully explained by a well-established chemical law.

The following experiment I have made a number of times, and always with the same result. Within a dwelling, a room is selected in which it is determined by careful tests that no ozone is present. If the atmosphere is dry, the vapor of water is added to it. In this, the ozonometers are exposed, and free chlorine allowed to escape and mingle with the air. If the amount of chlorine and vapor is sufficient, the characteristic discoloration of the paper will soon appear; and when it is less freely supplied, a longer time is required to produce the same effect. The discoloration is deeper and more rapidly produced by moistening the ozonometers with a little water. Now, what does this experiment teach us? It is this. The chlorine, by its strong affinity for hydrogen, decomposes the water, or the vapor, by which free oxygen is liberated in a nascent condition; this decomposes the iodide of potassium, setting free the iodine, which immediately unites with the starch, and produces the discoloration, which is considered indicative of the presence of ozone. In other words, ozone is generated from the decomposition of water through the agency of chlorine. In this particular, chlorine operates in the same way as electricity, each decomposing water and developing the phenomena of ozone by liberating the oxygen.

During the prevalence of thunder-storms ozone is unusually abundant in the atmosphere; and why? Because of the frequent and powerful discharges of electricity through an atmosphere filled with vapor and water. Much of the latter is readily decomposed, furnishing an abundance of nascent oxygen, or ozone. By this theory, too, we can understand why there is a greater amount of ozone observed in high elevations of the atmosphere when we approach the region of condensed vapor and the electrical influence of the clouds; and also, why it appears to be generally most abundant at night, when there is greater humidity in the atmosphere, near the surface of the earth, and a greater condensation of it upon the ozonometers.*

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* Scoutetten has demonstrated that nascent oxygen has the properties of ozone, which he attributes to an electrified condition of the oxygen, in accordance with his peculiar theory. The converse, that ozone is always nascent oxy-

According to the views which I have here expressed, I must regard the word ozone as an inappropriate name, or perhaps I should say we must learn to attach a new meaning to the word. It implies, as generally understood, a definite substantive of some kind, whereas it is only the manifestation of the property of something under peculiar circumstances, and can no more be isolated than can be an odor or a sound. The air in a certain state of vibration produces upon the organs of hearing the sensation of sound; and oxygen in a certain state of transition from one condition to another has the power of manifesting such properties as are expressed in the phenomena of ozone.

This view of the nature of ozone is not advanced from any desire to put forward a novel and unsubstantial theory, but because, with all due deference to the opinions of those from whom I have ventured to differ, and whose scientific attainments I do not presume to emulate, I believe it resolves the subject under a more comprehensive generalization, which is strictly in accordance with well-known scientific principles, and less arbitrary in character than any heretofore promulgated, and because it has a practical bearing upon the subject discussed in this report. If oils are charged with oxygen and exposed to sunlight, they contain oxygen still, and not ozone; and in the benefit observed from the administration of such oils, I find confirmation of my views in relation to the therapeutic properties of this element. In reference to the subjects I have considered in connection with the chlorate of potash, *the great remedy is oxygen;* and in whatever way it can be most readily administered and appropriated, that way is the best. We may yet find a better method than by giving the chlorate of potash, but at present I believe we have none. I doubt not, however, but such a discovery will be made, as the demand for it will become almost imperative, if the profession recognize in what I have offered a basis of treatment which is sound in theory and useful in practice.

It will be observed that the treatment of phthisis here recommended is based upon a peculiar theory of the pathology of this disease, namely, that the deposit of tubercles results from an imperfect elimina-

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gen, seems not to have occurred to him, nor any suspicion that it may always result from the decomposition of water. I also find a brief notice of another theory, closely approaching my own, that ozone may be formed of two or more equivalents of oxygen, one of which is given up in a nascent state to unite with bodies for which it has an affinity; but this will give no explanation of the fact that the presence of water is always essential as one of the conditions upon which the production of ozone depends.

tion from the system of the products of organic decay of the tissues of the body. This same view of the pathology of phthisis has recently been promulgated by Dr. Godwin Tims, in a work published in London, with the title of "*Consumption, its True Nature and Treatment.*" This work is noticed in the *Medical and Surgical Reporter*, of April 21st, under the caption of "*A New Theory of Phthisis.*" The author's pathological views are expressed in the following paragraph:

"The morbid condition which constitutes consumption, and which results in the deposit of tubercle, is an exaggerated activity of that part of nutrition called destructive assimilation, by which more atoms of nutrition are broken down, dissolved, and absorbed into the blood, than can be expelled by the excreting organs, until the blood has become so laden with the *débris* or *detritus* of the textures, as to precipitate and deposit it in the form of tubercle in favorable locations."

This new theory, as it is termed, is not of trans-Atlantic origin, as can be proved by the following passage from a paper on the medical properties of the chlorate of potash, published by the author in the *N. Y. Journal of Medicine*, in July, 1859. I quote from pages 24 and 25:

"Even tubercular deposits are known to consist principally of protein compounds, and we all know that the most effectual means of retarding or preventing their development consists in active out-door exercise in the pure air of the country, by which the system is supplied more freely with oxygen, *and the effete products of interstitial decay more rapidly removed.*

"Absolute deprivation of exercise, even in pure air, will lead to the deposit of tubercles. This has been well illustrated recently by Drs. Byford and Graham, of Chicago, in experiments upon animals; and confirmation of the opinion is found in the lungs of stall-fed cattle, the same as in the caged monkeys of Paris. The *rationale* of this can be readily understood when we consider that the circulation is of necessity retarded by want of exercise, and this involves *an imperfect removal from the system of such portion of the products of organic decay which results in the deposit of tubercles.* The same effect is more frequently the result of living in crowded and ill-ventilated apartments, where the inmates habitually breathe an atmosphere not only rendered impure by exhalations from the lungs, but also less vitalizing by containing a lower per centage of oxygen. Now, although the blood may circulate freely enough by means of exercise, yet it has not sufficient power, by means of a due proportion of oxygen, to carry on, in a healthy manner, all the functions of the body, *and in the same ratio, remove all its impurities. The most uniform effect of this retention is a*

tubercular deposit in the lungs or elsewhere; and this effect is most marked where the two causes are combined, viz., a lack of exercise while living in crowded and ill-ventilated apartments."

Whatever merit there may be in this "new theory of phthisis," as it has been termed, it is worthy of note, that it is not entirely original with Dr. Tims.*

In his therapeutics he has not yet, in my opinion, arrived at the *true* method of treatment, as the title of his book so confidently asserts. A part of his "combined treatment" by opiates, purgatives, animal oils, emetics, and blood-letting, undoubtedly results in a partial elimination of the *débris;* yet at the same time the purging and general depletion must remove from the system much that is essential to strength and health. Even this is not new. Thirty-five years ago blood-letting and emetics were the favorite remedies for phthisis with the celebrated Dr. Gallup, of the Castleton Medical College, Vermont, and I think this mode of treatment will hardly be revived successfully by Dr. Tims. Whatever of the impurities of the blood may be removed by such measures, a far greater loss of pure blood must accompany it; and in the reproduction of this, the previous loss will not guard against the reaccumulation of the detritus, but rather tend to

* It will be observed that this theory differs materially from the views advanced by Dr. Ellis, of Boston, and Dr. Gibbs, of Frewsburg, New York. The former regards the tubercular deposit as a "degraded condition of the nutritive material," which is incapable of assimilation, and expressed by the latter as "the albuminous material" which is "incapable of cellular development." According to this view, the tubercle is formed of elements *which never constituted a portion of the living organism;* whereas by the theory advocated by the writer, and recently promulgated by Dr. Tims, it is composed of the "*products of organic decay*"—of the elements once forming integral portions of the normal living tissues, the *débris* of which is thrown into the blood more rapidly than can be removed by the excreting organs. By the former the disease has its origin in a retarded or perverted hæmatosis, as the product of vitiated nutrition; by the latter, in an imperfect or retarded process of elimination of effete material, which previously existed in the normal structure of healthy tissue; a difference involving a still wider diversity in the chain of causation relating to the pathology of phthisis, as well as indications in prevention and treatment. This imperfect elimination I have attributed to a *deficient supply of oxygen,* in connection with insufficient muscular exercise. Not only Andral and Simon, but almost every modern pathologist, has recognized the fact which I admitted in the statement that "tubercular deposits are known to consist principally of protein compounds;" and it is a matter of some surprise that one acting in the capacity of critic and reviewer should have been so inaccurate as to state that I had claimed originality for this opinion, and to confound ideas so essentially different as my own and those expressed by Drs. Gibbs and Ellis.

its increase by the debility which must be the result of the depletion. On the contrary, how much more rational to strike at the root of the difficulty, by aiding nature in her own way in freeing the system from this accumulation of the products of interstitial decay through the agency of her own great remedy; and then guard against its return by proper hygienic treatment. This involves the loss of no strength by depletion, and imposes no tax upon the system by violent measures, but operates by increasing the supply of that element which is a natural tonic, and the most direct and powerful of all blood-depurants.

I cannot conclude this paper without calling attention to the fact that much of the chlorate of potash is very impure. We have the excellent authority of Dr. Squibb to this effect, and I have recently been convinced of the fact by some observations in practice. It is needless to say that only a perfectly pure article can be depended upon. With such views of the properties of the chlorate of potash, until a better method of administering oxygen is found, let this salt, which so readily parts with its large proportion of oxygen, be freely used, and, if I mistake not, a therapeutic agent will be found of incalculable value, and perhaps superior to any in our Materia Medica. The fact that a patient can take half an ounce a day for six weeks with impunity and marked benefit, is sufficient to disprove the statement of Dr. Osborn, who fancies he has discovered toxical properties in this salt, against the use of which he therefore cautions the profession. I have used it in many cases as freely as those now reported, and taken myself, as an experiment, half an ounce at one dose, without being able to discover any such properties; and I think I can safely say that they exist only in the imagination of Dr. Osborn. Its safety recommends it especially to our consideration, for in the whole range of our Materia Medica we have no agent so potent for good, and yet so harmless; and none, I will add, of such power and diversified application, the *modus operandi* of which can be so perfectly comprehended. Our knowledge of the properties of most of our medicinal agents is the result of general experience of an empirical character. We might know in advance that an alkali would correct an acid in the stomach, but who could foretell that opium would produce narcotism—that ipecac would vomit—scammony purge, or quinia cure intermittents? And after a knowledge of these facts as the result of a long experience, who can even now explain accurately and philosophically the relation of cause and effect as observed in these and hundreds of other instances? But in the administration of oxygen as a therapeutic agent, we know precisely how the indications are to be fulfilled by this

treatment. We are giving that element of "the breath of life" upon a constant and due supply of which depends the healthy operation of every part of the body—the agent of disintegration and elimination —the prime motive power of the human machine, "the leaden weight or bent spring which keeps the clock in motion." By the light of modern science, we are now looking beyond the great mass of heretofore ultimate facts, to discover, if possible, the laws and principles which govern their operations. Every advance made in this direction is true progress in medical science, by which it is pressing steadily onward in its career, with the unchanging laws of nature for its firm foundation.

NOTE.—With the foregoing views of the pathology of phthisis—by which its origin is referred to a deficiency of oxygen in the blood, whether resulting from breathing habitually an impure atmosphere, from insufficient bodily exercise, or physical incapacity to a full expansion of the chest—we cannot wonder at its almost universal prevalence and great fatality. Man was made to breathe at all times, by night as well as by day, *a pure atmosphere*, and to give due exercise to muscles which were made for active employment. But this demand of our physical organization is frustrated by a growing tendency to muscular inactivity, which the close confinement of many forms of business render almost unavoidable, and more especially by the breathing of air which is incapable of supporting life in a healthy condition, by reason of the bad ventilation of dwellings, places of business, and rooms for public assemblies. In treatment, the first indication is, of course, to avoid these evils as far as possible; and when more is required, the next will be to aid nature in her efforts to throw off the disease by supplying the blood with an additional allowance of oxygen by any method which may be found the most effectual. For this purpose the chlorate of potash is offered, not as a certain specific or panacea, but as a rational method of meeting the most important indication in the way of treatment which has been suggested by theoretical views, and to some extent practically confirmed by a limited experience.

The Physiology of the Circulation. A Course of Lectures delivered at the College of Physicians and Surgeons, New York, in the Fall Term of 1859. By JOHN C. DALTON, JR., M.D., Professor of Physiology and Microscopic Anatomy.

LECTURE IX.

(OCTOBER 4.)

Nature of Excretion and Secretion—New Substances formed in Secretion—Sugar in Liver—Experiment—Local Origin of Liver-Sugar—Formation of Sugar in Liver after Death—Experiment—Mode of Formation of Liver-Sugar—Glycogenic Matter—Its Conversion into Sugar—History of Discovery of Liver-Sugar—Different Opinions in regard to it—Objections to Glycogenic Function of Liver—Question of the Origin of Glycogenic Matter—Experiments—Investigations of Sanson—Of Poggiale—Of Pavy—Of Harley and Sharpey—Present State of the Question.

In the last lecture, gentlemen, we studied the phenomena of endosmosis and exosmosis, and the mode in which different substances pass through the animal frame by absorption and exudation. We found that these substances are taken up by the circulating fluids in different proportions, and with different degrees of rapidity; and, on the other hand, that the ingredients of the blood itself are exuded through the vessels, some in larger, and some in smaller quantity, while others still are retained within the circulation, and do not make their appearance in any of the secreted fluids.

Whenever a substance, therefore, exists in the blood, which is capable of passing through the animal tissues by exosmosis, it will be discharged with the various animal fluids. But as the same substance passes through different membranes with different degrees of facility, it will be discharged more rapidly by one organ, and less so by another. Thus, the different animal fluids, though all supplied originally from the blood, contain different proportions of watery, saline, and crystallizable substances.

Now this process, by which the ingredients of the blood pass out by the various glandular organs, is called the process of *Excretion*.

It takes place, as we have seen, whenever certain medicinal or poisonous substances are introduced into the blood. These substances at once pass out by exosmosis, and make their appearance in the different animal fluids, in varying proportions. Iodide of potassium, for example, appears immediately in the saliva, and soon afterward in the perspiration and the urine. Ferrocyanide of potassium, on the contrary, appears in the urine, but not in the saliva.

Bernard, in one of his experiments, injected into the jugular vein of a dog a solution containing, at the same time, seven and a half grains of ferrocyanide of potassium, seven and a half grains of iodide of potassium, and sixty grains of grape-sugar. Immediately afterward iodine was detected in the saliva, but neither the sugar nor the ferrocyanide could be found in this secretion at any time. On the other hand, all three substances passed into the urine, but at different periods, viz.: the ferrocyanide of potassium, in seven minutes; the grape-sugar in forty minutes; and the iodine only at the end of three hours.

The urea, also, which is produced in large quantity in the circulating blood, passes out rapidly and incessantly by the kidneys. This substance, therefore, is a natural ingredient of the urinary excretion, and does not usually appear in any of the other animal fluids. But if the action of the kidneys be suspended or impeded, so that the urea accumulates in the blood beyond its natural quantity, it may then be exuded from other parts of the circulatory apparatus, and appear in the perspiration, the saliva, and in serous effusions.

In all these cases, the constitution of the excreted fluid depends upon the varying endosmotic power of its different ingredients, and upon their varying affinity for the different glandular membranes.

But to-day, gentlemen, I wish to call your attention to another function connected with the circulation, still more peculiar and physiological in its nature. For all the animal fluids do not consist alone of the exuded ingredients of the blood. On the contrary, most of them contain, in addition, other substances peculiar to themselves, which do not come from the blood, but which are produced in the substance of the glandular organs. These peculiar substances, of new origin, are then mingled with the products of transudation, and thus form a compound animal fluid. This process, which we are now about to study, is known by the name of *Secretion*.

Excretion and secretion, therefore, are two essentially different phenomena. Excretion is a process of simple transudation, by which the ingredients of the blood pass out through the organic tissues in different proportions; while in secretion, new and peculiar substances make their appearance, and give a special character to the constitution of the secreted fluid.

In every secretion, accordingly, beside the watery and saline substances derived from the blood, there is a special and characteristic ingredient, which is formed by the glandular organ itself, and is found nowhere else. In the saliva, we find ptyaline; in the gastric juice, pepsine; in the bile, the resinous and crystallizable biliary substances

None of these matters pre-exist in the blood, but they all originate by a peculiar nutritive or secretory process in the tissue of the glandular membranes.

In all these instances the *materials* of the secretion are, of course, supplied by the blood. For the substance of the gland has no other source from which to derive its nourishment. It therefore absorbs from the blood watery, crystallizable, and albuminous matters, and appropriates them to the nutrition of its own tissues. But at the same time it compels some of these albuminous materials to undergo a process of transformation or metamorphosis, and converts them into substances of a different nature. It is in this way that new ingredients originate in the substance of an organ, as products of the secretory action of its glandular tissue.

Now, the products of secretion, thus formed in various parts of the body, are destined for various uses. Some of them are discharged upon the surface of the integument or into the alimentary canal; while others are taken up by the blood, and transported to distant parts of the circulatory system. Thus the constitution of the blood becomes altered in passing through such a glandular organ; for it not only loses the ingredients which transude into the glandular tissue, but it absorbs also the new substance which is produced in the texture of the organ.

At the same time a reverse alteration takes place in the blood, when this new substance is decomposed or discharged in other parts of the system.

This brings us, gentlemen, to a new, and very important element in the phenomena of the circulation, viz., the *alteration of the blood by the production and destruction of new substances in various parts of the circulatory system.*

Some of the most interesting of these phenomena are connected with the formation of sugar in the liver—a function which has of late years very much occupied the attention of experimental physiologists. This formation can be shown in a very simple manner. I have here a dog that has been kept under my own observation for several days. The dog is naturally a carnivorous animal, but in this instance pains have been taken to confine him strictly to a meat diet; and for at least a week he has had no food containing sugar, or any starchy substance capable of being converted into sugar. An hour or two ago he was fed with a moderate meal of fresh uncooked meat.

I will now kill the animal suddenly, by section of the medulla oblongata, and immediately proceed to place a ligature on the portal vein,

in order to confine the blood in this part of the circulatory system. The ligature is applied by the aid of an aneurism needle of peculiar construction, which you see here, introduced through a small opening in the abdominal walls. If the abdominal walls were first opened by a wide section, the natural support of the internal organs would be taken away, and the blood in the liver would be liable to regurgitate into the portal system. But it is our object to collect the blood from the portal vein, before it has passed into the hepatic capillaries.

The ligature is now applied, and the abdominal walls are widely opened, and the internal organs exposed. You observe the dark, purple color of the intestinal canal, owing to the obstruction and engorgement of the portal system of vessels by the ligature.

I will now proceed to collect a little of the portal blood by puncturing the vein behind the ligature, and catching the blood, as it escapes, in a porcelain capsule. It is immediately mixed with an excess of sulphate of soda, and a little water, and coagulated by boiling over a spirit-lamp. The fluid will afterward be filtered, so as to make a clear, colorless solution.

A portion of the liver is then taken, ground in a mortar, mixed with water and sulphate of soda, and the fluid mixture coagulated and filtered as before. Portions of the kidney, of the spleen, and of the muscular tissue are also taken, and treated in the same way.

All the fluids are now completely filtered, and we have therefore watery extracts of various tissue and organs, of the portal blood, and also of the tissue of the liver. I will examine them all in succession for the presence of sugar, by the application of Trommer's test.

In the extracts of the spleen, the kidney, and the muscles, no reduction of copper takes place, on using the test, and there is no reaction indicating the presence of sugar. The extract of the portal blood is equally without effect, and does not produce any decolorization in the solution of sulphate of copper. But on applying the same test to the extract of the liver there is at once, you observe, an abundant reduction of copper, and the solution is completely decolorized.

It is evident, therefore, that the tissue of the liver contained sugar, while none of this substance was to be found in the other organs, and none even in the portal blood by which the liver is supplied.

Now, this experiment might be repeated upon any other species of animal with the same result. In the horse, the ox, the goat, the sheep, the cat, the rabbit, the monkey, and in various kinds of birds, reptiles and fish, the liver has always been found to be highly saccharine in its ingredients, though no sugar were contained in other parts

of the body. In the human subject, also, the same condition exists. If the liver be examined in a man suddenly killed while in a state of health, or dead after a very short illness, it is found abundantly saccharine by the ordinary tests for such substances.

It is one of the most certain facts, therefore, that the liver is distinguished from most other organs by being constantly charged with sugar in a state of health.

The next important question to determine in regard to this subject is that of the origin of the sugar thus found in the liver. Is it introduced into the body from without, is it produced in some other part of the system, and deposited in the hepatic tissue, or is it actually formed in the substance of the liver itself?

It is almost evident, from what we have already seen, that it is neither introduced with the food, nor formed in other parts of the system to be deposited in the liver. For the liver contains sugar, in carnivorous, as well as in herbivorous animals; and the researches of Bernard have shown that the proportional quantity of sugar is as great in the carnivora as in vegetable feeders. At least there is no greater difference, in this respect, between the carnivora and the herbivora than between the different species of vegetable-feeding animals themselves.

For example, in the goat the per centage of sugar in the liver was found to be 3.89; in the ox 2.30, and in the sheep only 2.00. It was 1.69 in the dog, and only 1.68 in man, living on a mixed diet; while in the carnivorous cat and the vegetable-feeding rabbit it was precisely the same, viz., 1.94.

In this very animal, too, upon which I have just experimented, the sugar in the liver is abundant, and yet he has certainly had no sugar or starch with his food, for at least a week.

Beside, there is no sugar in the blood of the portal vein, nor in any of the other organs which we have examined.

We conclude, therefore, that the sugar found in the liver was produced in the organ itself, and did not come from any other source.

But there is another very remarkable fact in this connection, which was also first noticed by M. Bernard, which shows still more conclusively the local origin of the sugar found in the liver. It is that the liver, after being taken out of the body, and separated from all its connections, will continue to produce sugar for a certain length of time after death.

Here is a liver, for example, which I took this morning from the body of a recently killed dog. Immediately after its removal, I in-

serted the nozzle of an injecting-pipe into the portal vein, the other end of the pipe being connected, by a flexible tube, with the stop-cock of a hydrant. The liver was then injected with water from the hydrant, under a moderate force, the water penetrating through all its capillaries, and being discharged by the hepatic vein. At first the fluid discharged by the hepatic vein was colored with blood, and highly charged with sugar; but it soon became lighter in tinge and less abundantly charged with solid ingredients, and after a short time it passed from the vessels of the organ entirely colorless.

You now see the organ perfectly pallid and œdematous, from the watery injection. All the more soluble ingredients of the liver, including the sugar, have been washed away by the injected fluid, as we can very easily ascertain by experiment.

I cut off a portion of the liver, and after bruising it in a mortar, with water, coagulate the mixture as before, and filter. The filtered fluid, which is quite colorless, produces, as you see, no reduction whatever of the oxide of copper in Trommer's test.

This liver, therefore, contains no sugar. I will now leave it for twenty-four hours under cover, and at the next lecture will examine it again. In all probability, at that time, it will again give evidence of a saccharine ingredient.

Here, then, gentlemen, we have the essential characters of the function of *secretion*. The tissue of the liver, by virtue of its own properties, has the power of producing a new material, which did not exist beforehand in the blood, but which is formed entire in the substance of the organ itself. No sugar is to be found, in carnivorous animals, in the blood of the portal vein; but it nevertheless makes its appearance in the tissue of the liver, is absorbed thence by the blood circulating through the hepatic vessels, and is carried away by the hepatic vein. Thus we find that the blood is changed in constitution while passing through the organ. The blood of the portal vein, below the liver, contains no sugar; that of the hepatic vein, above the liver, is abundantly saccharine. This is not because the ingredients of the blood are directly transformed while passing through the hepatic circulation, but because the blood absorbs a new substance from the tissue of the organ, and thus acquires new properties while passing through its vessels. The sugar is supplied by the substance of the liver, and is absorbed from it by the circulating blood.

We have found, then, so far, that the sugar in the hepatic blood comes originally from the liver itself. Now let us see what is the

mode of its formation in the hepatic tissue, and what are the materials from which it is produced.

One of the most curious and interesting particulars in relation to this subject, which we again owe to M. Bernard, is the fact that, even in the liver, the sugar does not originate directly from the albuminous matters of the organ, but is produced from another substance, of anterior formation.

This substance, from which the sugar of the liver is produced, has a great deal of resemblance to the starchy bodies, and is called *glycogenic matter*. It may be extracted from the hepatic tissue by a very simple process. The liver is taken from the body of the animal, immediately after death, cut into small pieces, and then thrown at once into boiling water, where it is allowed to remain for five or six minutes. This is in order to coagulate the albuminous substances of the liver, and prevent their exciting a sort of fermentation which would destroy the substance sought for. The coagulated pieces of liver are then taken out, and pounded in a mortar with a little cold water, until the whole substance of the organ is reduced to a semi-fluid pulp or magma. This is to be diluted with distilled water, gradually heated to the point of ebullition, and boiled steadily for an hour. It is then pressed and filtered. The filtered fluid comes through with a slightly yellowish color and an opaline tinge.

This fluid contains the glycogenic matter, extracted by boiling water from the tissue of the liver. It may be precipitated by the addition of alcohol in excess, or of glacial acetic acid, as it is insoluble in both these substances. After precipitation, it may be purified by washing with alcohol, and by treatment with animal charcoal, and afterward dried and preserved. Here is a specimen of glycogenic matter which has been extracted in this way. You see that it is a nearly colorless, dry, powdery mass, very much like some kinds of starch or dextrine. It is soluble in water, making an opaline liquid, and may be again precipitated by alcohol, or strong acetic acid. It contains no nitrogen, and consists of carbon, hydrogen and oxygen, in the following proportions, viz.:

$$C^{12}\ H^{12}\ O^{12}.$$

Its constitution resembles so much that of the starchy matters, that it has been sometimes known by the name of *animal starch*, or *animal dextrine*.

But the most important property of this substance is, that it may be instantly converted into sugar by the action of a ferment. The glycogenic matter, alone, does not reduce the oxide of copper in Trom-

mer's test, nor answer to any other of the tests for sugar. But if it be dissolved in water and placed in contact with any albuminous ferment, such as the animal matter of the saliva, or of the intestinal juices, it is at once converted into sugar, and will reduce the oxide of copper without difficulty. The blood itself contains a ferment which is capable of producing the same change, at the temperature of 100° F., and a similar one is also to be found in the tissue of the liver.

There are two stages, therefore, in the secretion of sugar by the liver. The first consists in the production of glycogenic matter by the hepatic tissue. In the second place, this glycogenic matter is soon afterward converted into sugar, by a process of catalysis, under the influence of an animal ferment, also contained in the liver. Lastly, this sugar is absorbed by the circulating fluids, and carried away by the blood of the hepatic vein.

Now, since the first discovery of this fact by M. Bernard, in 1848, the ideas and opinions of the profession in regard to it have passed through the most curious changes. It will be interesting to trace the history of these changes for the last ten or twelve years, and to see how they have led to our present convictions.

The most doubtful point, in relation to the whole matter, was, of course, that of the *internal or external origin* of the sugar found in the liver and the blood of the hepatic vein. Bernard showed beyond a doubt that the substance which he discovered there was really sugar. For he not only distinguished it by the most ordinary tests, such as that of carbonizing by the action of sulphuric acid, turning brown by boiling with potassa, and reducing the oxide of copper in Trommer's test; but he placed its identity beyond question, by subjecting it also to the absolute and unvarying test of fermentation. The extract of the liver, mixed with a little yeast, and kept at a temperature of from 70° to 100° F., fermented readily. The gas, which escaped in bubbles, was collected, and found to be carbonic acid; and the remaining fluid was concentrated by distillation, and the distilled portion found to contain alcohol. It was certain, therefore, that this new ingredient of the hepatic tissue belonged to the group of saccharine substances.

Bernard also showed very strong evidence in favor of the internal origin of this sugar. He ascertained the percentage of sugar, for example, existing in the liver of animals fed on different kinds of food, and found that the quantity of sugar was as great in dogs fed upon meat alone, as in those fed upon meat and bread, or even upon starch and sugar. He kept two dogs, also, during a very long time, exclu-

sively upon an animal diet, in order to see whether the liver would still contain sugar. One of these animals was kept for three months upon cooked meat, (boiled sheep's heads,) the other for eight months upon scalded tripe. At the end of that time they were killed, and the liver in each instance contained an abundance of sugar.

But it was thought that the sugar in these cases might have been introduced with the food before the commencement of the experiment, and might have remained condensed in the tissue of the liver during the whole time of its continuance. Bernard, however, showed that this was not probable, by the following instance:

Nine rats were caught in the sewers of Paris. Three of them were at once killed, and their livers found to be highly saccharine. The rest were then kept entirely without food for four days. At the end of that time three of them were killed, and their livers, upon examination, were found to be nearly destitute of sugar, only slight traces of this substance being discovered, too minute for its quantity to be estimated. The sugar which existed in the livers of these animals, therefore, at the time of their capture, had disappeared during their four days' starvation. The remaining three rats were then supplied with an abundant meal of raw beef, and when killed, six hours afterward, their livers found to contain an abundance of sugar.

It was concluded from this that the sugar of the liver was not derived from any saccharine ingredient of the food, but was formed in the body itself, and was of strictly internal origin.

But two very curious objections were soon after made to Bernard's experiments and conclusions, both of which appeared about the same time, in February, 1855. One of them was made by M. Figuier, the other by M. Longet. M. Figuier maintained that the herbivorous animals received sugar and starchy substances with their food; that the sugar so introduced was taken into the blood, and circulated throughout the vascular system; that, in point of fact, traces of sugar could be readily detected in the blood of such animals; that the carnivora, in eating the flesh of the herbivora, received sugar with their food, since the muscular tissue of the herbivora contained blood, and their blood contained sugar. So that, according to M. Figuier, an animal kept for three or eight months on the flesh of sheep or cattle was receiving all that time, with the food, the very substance which was afterward found in his liver; and such an experiment was no proof that the sugar of the liver was formed internally.

Bernard, however, showed conclusively that this objection was not valid. For though the blood of the herbivora does readily contain

traces of sugar, and though the muscular flesh, taken warm and dripping from the body of the animal, also contains a small quantity, this is not the case with meat which has been kept for a short time, for the sugar in the blood of the muscles at the moment of death is very soon decomposed, and is not reproduced. Meat prepared for the market does not, therefore, contain sugar; still less, meat which has been cooked by boiling, or in any way drained of blood; and particularly, the kind of meat with which his animals had been fed, did not, in point of fact, contain any trace of sugar, and yet this substance was abundant in their livers at the end of several months.

M. Longet, on the other hand, announced that sugar might exist in the blood of the portal vein, and yet not be discoverable by Trommer's test, owing to the presence of *albuminose*, which interferes with the reduction of the oxide of copper. That being the case, he maintained that the sugar found in the liver was supplied to that organ by the portal vein; and that the only reason why it had not been discovered in the portal blood was, that its reaction was interfered with by the presence of albuminose.

But this objection of M. Longet was no better founded than the other; for a very simple examination will show that the liver may be charged with sugar, and the portal blood destitute of it, when the portal blood contains no ingredient capable of interfering with Trommer's test. For if a little grape-sugar be added to the portal blood, which does not, by itself, cause any reduction of the oxide of copper, the extract of the same blood will afterward give an abundant precipitate by Trommer's test.

Beside, Bernard never trusted exclusively, in his decisive experiments, to Trommer's test, but employed the test by fermentation, as well. The extract of the liver will ferment rapidly, when no fermentation can be excited in the extract of the portal blood.

Other experimenters also published results confirmatory of Bernard's discovery. Lehmann communicated to the French Academy a list of experiments of his own, on dogs, either entirely deprived of food or fed exclusively with meat, in which the blood of the portal vein was destitute of sugar, while that of the hepatic veins contained it in perceptible quantity. M. Leconte and M. Poggiale came separately to the same conclusion; and M. Moleschott was led to the belief, by his own experiments, that the sugar of the liver originated in the organ itself, and did not come from without. All these investigations were published in March and April, 1855.

M. Figuier, however, made a second communication to the Acade-

my, in which he still maintained that the sugar of the liver was brought to it by the blood of the portal vein, and was derived originally from the products of digestion in the intestine.

These experiments and conclusions of M. Figuier were referred to a special committee of the Academy, consisting of M. Dumas, M. Pelouze, and M. Rayer. This committee, after having examined the subject with care, made a report at the session of June 18th, 1855, in which they did not confirm M. Figuier's experiments, but, on the contrary, found that no sugar was to be discovered in the portal blood of a dog fed on raw meat, while it was easily detected in the blood of the hepatic vein. They accordingly arrived at the conclusion that "all the facts announced by M. Bernard had been verified by the committee," and that, up to that time, "the doctrine of the glycogenic function of the liver appeared unshaken."

So far, then, it seemed very certain that the liver is the real source of sugar in the animal economy, since this substance is an invariable constituent of the hepatic tissue, and does not exist in the blood with which it is supplied.

During all this time, the greatest interest was excited among physiologists everywhere, in regard to the subject. Many others, beside those whom I have mentioned, took part in the discussion, some sustaining the views of M. Bernard, others in opposition. But the general verdict was in favor of his conclusions.

In 1857, however, a new face was put upon the matter by the discovery of the glycogenic matter, by M. Bernard himself.

This discovery at once made it necessary to go over again the entire question of the internal or external origin of sugar; for the glycogenic matter of the liver resembled so much the starchy bodies, that it seemed not at all impossible that it might have a vegetable origin. As it was disputed beforehand, therefore, whether the *sugar* of the liver had been introduced with the food of herbivorous animals, so it was now a question whether the *glycogenic matter*, the direct source of the liver-sugar, might not have found its way into the animal organism in a similar way.

M. Bernard maintained that the glycogenic matter was an animal substance; that it was formed in the liver; and that it could not be found in other organs, nor in the blood of the portal vein.

But there were others who thought differently.

M. Sanson published a memoir in Dr. Brown-Séquard's *Journal de Physiologie*, for April, 1858, in which he maintained that the glycogenic matter was a normal constituent of many other parts of the body

beside the liver. M. Sanson was a professor in the Veterinary School of Toulouse, and most of his experiments were made on horses and cows. He examined the spleen, the kidneys, the lungs, the muscular flesh, the blood of the jugular vein and of the portal vein, and arterial blood, by the ordinary process, and found that they all contained glycogenic matter in perceptible quantities. The same substance was detected in the portal blood, and even in the blood of the jugular vein of a dog, fed for some days exclusively on cooked meat. He concluded that the glycogenic matter, therefore, was nothing else than vegetable dextrine, taken with the food of the herbivorous animals, and that the same substance existed in the flesh which forms the diet of the carnivora.

This memoir of M. Sanson was referred to a committee of the French Academy, consisting of MM. Bouley, Poggiale and Longet. These gentlemen went over the whole ground very carefully, and repeated all the experiments together with M. Sanson. They reported, however, against his conclusions. Their decision was,

1st. That in dogs fed exclusively upon meat the glycogenic matter is to be found only in the liver; and that this substance, therefore, in all probability, is formed in the hepatic tissue.

2d. That glycogenic matter is very abundant in the liver of herbivorous animals. It is not to be found in the other organs, unless these animals have been fed upon a diet rich in starchy materials.

3d. In a large number of experiments, glycogenic matter was found in butchers' meat only once. In other experiments, it was found constantly in the flesh of horses; but this fact was not regarded as proof that glycogenic matter was in all cases derived from the food.

This report was published in the *Journal de Physiologie*, for July, 1858.

I have also been led to the same conclusions by my own experiments upon carnivorous animals. In order to ascertain whether any substance existed in the portal blood, capable of being converted into sugar, it was examined as follows:

Experiment, September 30th, 1858.—A healthy dog, of medium size, was kept for three days exclusively upon the fresh uncooked meat of the bullock's heart. He was then killed, by section of the medulla, three and a half hours after feeding. One fluid ounce of the portal blood was then immediately collected, by the ordinary method, and defibrinated by stirring. One-half was at once coagulated by boiling with water and an excess of sulphate of soda, and the filtered extract subjected to Trommer's test for sugar. No sugar was present. The

remaining half was kept, for two days, exposed in a glass vessel, at a mild temperature. At the end of that time, it was examined for sugar, by Trommer's test, in the same way as above, but no indications of the presence of sugar were manifested.

The liver of this dog was used for the extraction of glycogenic matter. It was cut into small pieces, and coagulated by boiling water—then reduced to a pulp by pounding in a mortar, boiled with a small quantity of water, pressed, and filtered. An abundance of glycogenic matter was precipitated from the filtered fluid, by the addition of alcohol in excess. It was yellowish in color, and dissolved readily in pure water, giving to the solution a strong opaline tinge. The watery solution gave no blue color on the addition of iodine, but only a clear, deep, maroon-red.

The solution of glycogenic matter showed no traces of sugar when subjected to Trommer's test; but on being mixed with saliva, and placed in a jar of water, at the temperature of 100° F., at the end of four minutes it was found to contain an abundance of sugar, and reduced the oxide of copper with great readiness.

The saliva used in this instance was very active, and, in a counter experiment, converted boiled starch into sugar in less than a minute.

In order to ascertain whether any glycogenic matter existed in the meat upon which these dogs were fed, the following experiment was performed:

Experiment, Oct. 6th, 1858.—One pound of the fresh lean meat of the bullock's heart was taken, cut into fine pieces, coagulated by boiling water, then drained and washed, and afterward boiled for one hour in a small quantity of distilled water, just sufficient to bathe its substance. It was then drained, pressed in a linen bag by a porcelain press, and the fluid of decoction filtered through paper. The filtered fluid was nearly two ounces in quantity, of a light yellowish color, nearly clear, and with hardly anything of the deep, opalescent appearance which is presented by a decoction of the liver under similar circumstances. The fluid was acid in reaction, with a specific gravity of 1008. It did not show any signs of sugar by Trommer's test.

Treated with five times its volume of alcohol, at 95 per cent., it let fall a copious white precipitate, which did not contain any glycogenic matter, as shown by the following trials.

The precipitate was separated by filtration, well washed with pure alcohol, and dried over the water-bath. Redissolved in distilled water, it did not give any bluish or red color, on the addition of iodine. Mixed with an equal quantity of saliva, and kept at 100° F.

for twenty minutes, it did not give any indications of sugar by Trommer's test.

Another portion of the same saliva, mixed with boiled starch, caused the appearance of sugar in less than two minutes.

I have also found that, in dogs fed upon meat which contains no glycogenic matter, this substance may be detected in the liver, but not in other organs.

Experiment, Oct. 12th, 1858.—A healthy dog was fed for eight days exclusively upon the fresh meat of the bullock's heart, and then killed by section of the medulla oblongata.

The liver, spleen, kidneys, and one lung were taken out and cut up into small pieces. A portion of each organ was then examined in the ordinary way, by Trommer's test for sugar. The liver alone contained this substance, the other organs showing no trace of it.

The remainder of the organs were then separately coagulated by being placed in boiling water, then drained, reduced as nearly as possible to a pulpy condition, by bruising in a mortar, and the pulp boiled for an hour in a small quantity of distilled water. The decoction was then filtered, and precipitated by alcohol, and the precipitate dissolved in water. In neither case did this watery solution show any trace of sugar by Trommer's test. The remainder of the solutions were then placed in contact with saliva at 100° F. At the end of ten minutes the extract of the liver showed abundance of sugar, while that from the other organs gave no reaction whatever with Trommer's test.

It is plain, therefore, that the substance from which the sugar is produced, as well as the sugar itself, is formed in the liver, and is not derived from any other source.

But the most singular opinion yet advanced in regard to this subject, was brought forward in the latter part of 1858, by Dr. Pavy, of London, in a communication to the fourth volume of Guy's Hospital Reports. Dr. Pavy confirmed the observations of others so far as to establish, that the glycogenic matter was confined to the liver, and did not exist in other regions. But he was led to believe that this glycogenic matter was never converted into sugar during life, but only after death, by an altogether accidental and unnatural transformation. He maintained, therefore, that during life it was only the glycogenic matter, not sugar, that was produced in the liver, and regarded the production of sugar as a purely post-mortem occurrence. He arrested the conversion of glycogenic matter into sugar, in some instances, by injecting the liver of the animal before death with a solution of potassa, and, in other cases, by plunging the liver immedi-

ately into a freezing mixture. Under these circumstances, he found no sugar, either in the blood of the hepatic vein or in the tissue of the liver, but only glycogenic matter.

The post-mortem production of sugar, however, according to Dr. Pavy, must take place with the greatest celerity, since, as every one knows, a minute or two is all the time required to take out the liver from an animal instantaneously killed; and yet the liver and the hepatic blood, under these circumstances, always contain an abundance of sugar.

These views have not been generally adopted or corroborated.* Dr. Geo. Harley and Prof. Sharpey, of London, performed a number of experiments, which were reported to the Royal Society, in February, 1860. Their results led the experimenters to the following conclusions:

1. Sugar is a normal constituent of the blood of the general circulation.

2. The portal blood of an animal fed on a *mixed* diet contains sugar.

3. The portal blood of a *fasting* animal, as well as of an animal fed solely on *flesh*, is devoid of sugar.

4. The livers of healthy dogs contain sugar, whether their diet be animal or vegetable.

5. Under favorable circumstances, and with proper precautions, saccharine matter may be found in the liver of an animal (a dog) after three entire days' rigid fasting.

6. The sugar found in the bodies of animals fed on *mixed* diet is partly derived directly from the food, partly formed in the liver.

7. The livers of animals restricted to flesh diet possess the power of forming glycogene, which glycogene is, at least in part, transformed into sugar in the liver.

8. As sugar is found in the liver at the moment of death, (even when the plan of freezing it has been strictly attended to,) its presence cannot properly be ascribed to a post-mortem change, but is to be regarded as the result of a natural condition.

These, gentlemen, are the phases through which the history of this remarkable discovery has passed thus far. At the next lecture, we shall study the mode in which the sugar disappears in the circulation, and the other similar changes which affect the constitution of the blood.

* I introduce a notice of Dr. Harley's experiments here, in order to bring the account of the subject up to the present time. J. C. D.

Report of a Trial for Criminal Abortion. By C. P. FROST, A.M., M.D., of St. Johnsbury, Vt.

An individual, a native of Ireland, styling himself Dr. W. H. M. Howard, and professing to be an English surgeon of great renown, was arraigned at the January Term (1859) of the Orange County (Vt.) Court, upon an indictment charging him with producing criminal abortion on Miss Olive Ashe, and also with causing her death thereby.

From the evidence adduced on trial, (the minutes of which have been kindly furnished me by the presiding Judge, Hon. James Barrett,) it appeared that Miss Ashe went to the establishment of Howard, at Bradford, Vt., about the middle of January, 1858, for the purpose of having abortion procured, supposing herself pregnant by a young farmer, by whom she had been employed as a servant during the previous summer; that a bargain was struck between the reputed father of the child and Howard, by which he was to perform the desired service for the sum of $100.

Her twin-sister, who was with her during the whole of her stay at Howard's, testified that the doctor operated three times with instruments. These instruments she could not describe minutely, nor could she tell the number used. As the result of the first operation, in which he introduced one or more instruments into the body of her sister, there was a discharge of water, which she said lasted two or three hours or more. On the next forenoon he operated again; used two or three instruments; sister made no great outcry, but complained, and gave other evidences of considerable pain. This operation was followed by a discharge of quite a quantity of blood. At night of the same day he operated a third time. Sister did not sit up after the second operation. He used instruments at this time, and also introduced his hand. The result of this operation was the delivery of the child, which was about two-thirds grown. Flowing continued a few days. This last operation was Saturday night, and sister lived till the next Friday evening, January 29th. The last two or three days of her illness were attended with delirium, picking the clothes, &c.

The medical witness first called was C. P. Frost, M.D., of St. Johnsbury, Vt. He testified that he had been in practice two years; that at the request of Government he made a post-mortem examination of a body said to be that of Olive Ashe, on the 4th of February, 1858, six days after death; found the body plump; well nourished in its general condition; under the eyes was a dark-yellowish areola; mammary glands were well developed, and milk could be obtained from

both breasts; dark areola about the nipples; abdomen distended; between the thighs was found a quantity of cotton soaked with a purulent discharge from the vagina; vulva enlarged and swollen; vagina dilated, so that the hand could be passed into it without much difficulty; mucous membrane of vagina of dark-blue color, and covered with pus. On making incision from sternum to the pubes, found the subcutaneous adipose tissue ½ inch thick; the peritoneum was in its normal condition, except that the vessels between the uterus and bladder were increased in size and number; the uterus was 5½ inches in length, 4½ wide, and 1½ thick, and weighed one pound; os uteri 1½ inch in diameter; cervix uteri in a state of slough; the mucous membrane and muscular tissue being nearly destroyed; one deep slough on the anterior surface was not entirely detached; the body of the womb was in a healthy condition; placental mark well defined upon the right anterior and lateral portion of the fundus, and covered an area of six or eight square inches, and presented large open-mouthed sinuses; other organs of the abdomen perfectly normal; also those of the thorax; head not examined. The direct cause of death, in the opinion of the witness, was inflammation and sloughing of the cervix uteri, perhaps accompanied by hæmorrhage. The inflammation and sloughing were caused by violence in the use of instruments applied to the neck of the womb in the attempts to induce abortion. Believed that instrumental violence was used, because of the depth of the slough at certain points, extending to the entire destruction of mucous membrane and muscular tissue. The inflammation and sloughing had obliterated or changed the direct evidence of violence. From the appearance of the womb, he judged the wounds were made at least three days prior to death, and probably a week or more.

On cross-examination, witness gave as the reason for the opinion that the mark called placental was really such, the increased vascularity of the uterus, and the number and size of the sinuses. He gave a description of polypus, also of the hydatid, and asserted that their attachment to the uterus differs unmistakably from that of the placenta. He knew too little of the corpus luteum to be able to distinguish in this case whether it was that of menstruation or pregnancy. He was fully satisfied in his own mind that something the result of impregnation had been expelled from this womb a short time previous to death. The increased vascularity of that organ could be accounted for upon no other supposition than that of pregnancy.

Selim Newell, M.D., of St. Johnsbury, Vt., was next called, and testified that he had been in practice thirty years; that he saw the

uterus, said to be that of Olive Ashe, on the day after the autopsy, and gave it a thorough examination. On the right anterior portion of the fundus was a well-defined mark, differing in color from the remainder of the lining of the uterus. It had a rough surface. Some portions were easily detached, and others were more adherent. There were well-developed sinuses with open mouths, and the uterine wall was thickest on the side on which this mark was found. To his mind, it indicated the point of placental attachment; could account for the appearances there found in no other way. His belief is that there are no marks of real placental attachment in case of the so-called moles.

The uniform practice of physicians in cases where it is deemed necessary to procure abortion, is to call in counsel, that they may be shielded against the charge of criminal abortion. Mechanical means are proper to be used in procuring abortion. No medicine or drug is regarded as a sure abortive.

He believed the appearances of injury about the cervix uteri in this case were due to mechanical violence. He knew of no other satisfactory way of accounting for those appearances. The wounds inflicted would cause hæmorrhage; uncertain to what extent—possibly sufficient to produce death.

On the cross-examination, he stated, after a lengthy description of the various morbid growths found in the uterus, that none of them have a placental attachment, though after the removal of some of these appearances, somewhat similar to those found after removal of a placenta, might be met with. These growths would cause enlargement of the abdomen and breasts, and might produce the areola about the nipples. They may produce any one of the symptoms of pregnancy, but he would hardly think *all* in the same case.

Ulceration of the cervix uteri in ordinary disease of the womb is not deep. Cancerous disease would produce deep ulcers. Parturition may cause laceration of the cervix, which, if attended by cold, &c., may result in death. It might possibly result in the formation of a cavity as a consequence of sloughing. Inflammation from mechanical injury to the cervix might occur in thirty-six hours. Health of patient and mental condition might modify it somewhat. He thought the marks existing on this womb were caused by inflammation, and might be produced in from seven to ten days after the injury was inflicted.

The first medical witness called by the defence was E. E. Phelps, M.D., of Windsor, Vt., who testified that he had been in the practice of medicine since 1824, and had been Professor of Theory and Practice and Pathological Anatomy since 1840. He described the mor-

bid growths found in the uterus under three heads: 1. Fibrous growths. 2. Polypi. 3. Cysts.

The expulsion of these foreign bodies from the womb may produce laceration of its neck and mouth. Disease of the os and cervix uteri occurs as the result of inflammation, and is found most frequently on the exterior. Ulcers of various forms are found. Whitehead mentions one oblong and quite deep. *Theoretically*, the rounded may be quite deep, and not *necessarily* the result of mechanical violence. Ulceration, either external or internal, may be a cause of abortion. He had examined the womb produced here by Dr. Frost; had examined the ovaries. Said he was not sufficiently conversant with the corpus luteum after the preparation had been kept in alcohol to derive any aid from it in determining whether pregnancy had existed or not.

From the appearance of the womb, and the description given by Drs. F. and N., he did not think a case of pregnancy has been made out. His reason was, that any evidence furnished by the uterus itself in its present condition was worth but little to him in regard to that, because the preparation had been kept so long in alcohol—a year. Had heard the description given by Drs. N. and F. of the placental mark, and had examined those marks. The evidence of the placental mark should be pretty clear. That evidence to him, through them, is secondary—a secondary matter. All the appearances on the womb, and those described by Drs. N. and F., may be accounted for on some other supposition than that of pregnancy. A fibrous growth may have been removed spontaneously or by mechanical means. It is possible that the marks on the neck of the womb, described by Dr. F., may have arisen from some other cause than mechanical violence. The idea was not perfect in his mind as to what they were before the uterus was put in alcohol. Had it been a more recent preparation, he could have judged better. It is *possible* they may be the result of disease. It would be very difficult to distinguish with certainty between sloughing from ulceration and from injury by mechanical means. A debilitated state of the system would tend to produce ulcers. Whether it would increase their depth he was uncertain; it might tend to do so.

Depressing passions and emotions are causes of miscarriage. If it occurs from any of these causes, the life or death of the fœtus would depend on the duration of the cause. If the depressing emotion existed for a long time, the fœtus would be more likely to be dead than if the cause of miscarriage acted suddenly. If the cause existed for several months, *theoretically* he would think the chances between a dead and living fœtus nearly equal. From what he had heard testified by

the sister, relative to what occurred at Dr. Howard's as to operations, pain, symptoms and appearances, he had no evidence tending to convince him that a living child had been born, excluding her testimony that she saw one. He was not satisfied that a *living* child was born, if a child was born. He saw nothing in her evidence that convinced him that there was a living child in utero at any time. If a child was in utero, he saw no evidence that it was living at the time Howard performed his first operation.

The water described as passing out on the first operation may be accounted for by supposing the existence of a hydatid, which was ruptured by the introduction of an instrument. The bleeding at the second operation may be accounted for in many ways. It might result from labor. If there was a fœtus in the womb, and labor was progressing, there would be more or less flowing before delivery. From such flowing it does not follow that mechanical violence had been used. If disease existed about the cervix uteri, expulsion of a putrid fœtus, passing over the ulcers, would be likely to make them worse than before, and might result in slough. Delivery of a dead fœtus would be less likely to irritate the neck of the womb than a living one.

On the cross-examination, Dr. Phelps said that if called in two or three days after the death of a woman, an examination would at times enable him to determine *conclusively* whether she had been delivered of a child eight or ten days previously; at times he could not so determine. If we were to find portions of the placenta attached to the uterus, or a portion of the cord within it, there would be no manner of doubt that there had been a child. Or in another case we might find a large red mark on the walls of the uterus, corresponding in size and form to the attachment of the placenta, and the uterine cavity filled with blood; we could then determine conclusively that delivery of a child had taken place a few moments before death. It would be possible to decide with certainty, by post-mortem examination, of a woman ten or twelve days after delivery, whether she died in consequence of parturition.

In the uterus presented in this case he saw no evidence of hydatids showing that they certainly were present. Nothing had been given in court that satisfied him as to the cause of death. He could not say, from the evidence, that she did not die in consequence of being delivered of a child. From an examination of the womb, he did not see evidence that there had been a polypus or a fibrous tumor. In case of post-mortem examination, eight or ten days after delivery, he

would, as a medical man, come to a conclusion whether a child had been born or not; but if his brethren—six or eight—should all engage in it, he would certainly expect all would not agree. He would not expect the evidence to be so conclusive that half a dozen, or even four men, would agree. This would not be the fact in the majority of cases, for they would be clear. In an autopsy, undertaken for the purpose of determining whether delivery of a child had taken place, it is highly probable that we should meet with appearances which would not lead several medical men to the same conclusion. For example, in a case where we are thrown entirely on the character of the placental mark, we should have to rely on characteristics with which we are not familiar. We have read about them, and our imagination may have misled us. The value of our opinion would depend on our familiarity and experience with these things. As a general law on post-mortem examinations, after delivery, there would be evidence of increased vascularity of the uterus, thickening of its walls, and some unnatural appearances about its os, and some placental marks in some degree of perfection. With these conditions, and without finding fragments of morbid growth, he would have no doubt that it was a case of pregnancy. Beneath the placental attachment we should find the vascularity very great. From the vascular condition of the wall of the uterus, at the point of attachment, he did not think he could decide whether a placenta or a hydatid had been attached. Did not think he could distinguish between the two from vascularity alone. The quantity of blood-vessels necessary to supply blood to the fœtus would be much larger than would be required for the hydatid.

He saw no conclusive evidence of ulceration about the uterus exhibited in this case. He could not say whether sloughing from bruise or injury would leave such a cavity as is shown in this case, the preparation has been kept so long in alcohol.

In the case described by Drs. N. and F., and the uterus, as here presented, without having seen its contents, it was impossible for him to tell what it contained. All the signs of pregnancy derivable from a post-mortem examination of the body, except a perfect placental mark, may exist from other causes.

S. J. Allen, M.D., of W. R. Junction, Vt., was also examined; his testimony did not differ materially from that of Dr. Phelps.

After a trial, lasting nine days, the jury brought in a verdict of guilty on the charge of abortion, and not guilty on that of manslaughter. After a hearing before the Supreme Court, on exceptions to the ruling of the Judge, Howard was sentenced to two years' imprison-

ment in the State Prison, where he is now serving out his sentence. An indictment for criminal abortion and manslaughter in the case of a young woman, who died two days previously to Miss Ashe, was also found against him.

A Case of Gun-Shot Wound; Bullet found in the Wall of Right Ventricle of the Heart Eighteen Years after the Accident. By G. B. BALCH, M.D.

A short history of the case is as follows: In June, 1842, an Irish boy, by the name of John Kelly, received an accidental shot in his right shoulder; the ball passed through three inch-boards before it struck him. A surgeon was called, who probed the wound, and found the ball lodged nearly under the inner third of the clavicle. The ball entered the shoulder through the upper border of the trapezius muscle, about an inch and a half or two inches from the acromion process. There was not much hæmorrhage at the time, and the surgeon did not deem it prudent to remove the bullet, and in about six weeks the boy was able to be at work. This accident occurred at Chatham Four Corners, Columbia Co., N. Y. In 1844 Mr. Kelly came to this county, (Clinton,) where he has since resided.

Fourteen years ago he was taken very dangerously ill with pneumonia, accompanied with a very severe and irregular palpitation of the heart. Dr. Terry, who attended him at that time, says he did not expect his recovery. Ever since that sickness his heart has shown symptoms of organic disease, at times beating in such a tempestuous manner, that one standing ten or fifteen feet from him could see its action very distinctly. Ever since he was shot he has had strabismus, and, at times, inflammation of the right eye.

His last sickness was caused by his going into the water, ten days before his death, and taking a severe cold; his heart then commenced its actions with redoubled fury, accompanied with dysphonia, and severe pain in his shoulders and arms. His right arm became purple and cold before death.

On Friday, June 15th, 1860, I made the post-mortem examination, by request of Dr. Terry, who was his attending physician. The autopsy revealed a condition of things I did not expect. The right subclavian artery was filled with ossific matter at the thyroid axis; the other arteries were healthy. The right internal jugular and subclavian veins were enlarged; the right external jugular was closed

near its union with the internal; I found the remains of the vessel where it entered the internal jugular.

The upper lobe of the right lung was congested. There were no tubercles in the lungs, but there was considerable pleuritic adhesion.

The heart was enlarged, and undergoing fatty degeneration. The pericardium was very adherent; so much so that I could not separate it from the heart, without cutting either one or the other. At the lower part of the right ventricle I felt a hard lump. I passed my finger into the right ventricle, and found the lump to be in the wall of the ventricle, near its lower part. I then cut with my scalpel from the outside down upon the lump, and found it to be a leaden bullet, slightly flattened.

Now the query arises, How long had this bullet been in the heart? I will not advance any theory of mine; I think the facts of the case tell the story, without any theorizing.

Abstracts and Translations from Foreign Journals. Prepared expressly for the MONTHLY.

THE UTILIZATION OF EVERYTHING FOR FOOD IN CHINA.

BY THE ABBE LE NOIR.

The progress of Chinese civilization cannot be compared with that of Europe. The Mongolic progress takes place, or rather has taken place, with a special characteristic of slowness that has been of value. Agriculture, for example, has so progressed in this immense empire, through the system of small patriarchal estates, that no country on the globe can contend now with China in the abundance of its agricultural products, although this does not prevent, in consequence of the enormous population, (if the census were known, it would probably reach four hundred millions,) an occasional destruction by famine, which has reached even to a hundred thousand victims.

The Chinese have long recognized the necessity of losing nothing; they have conquered natural repugnances, and introduced many natural products, which we reject, into their ordinary food. The lower classes, first, have employed substances despised by the rich; for with the former, children of necessity, all movements of progress take birth, which are afterwards adopted by every one. * * Dogs' flesh is considered, in Europe, as the worst kind of meat; it is called unmasticable. The Chinese have decided otherwise; they fatten dogs that are getting old and eat them; the butchers' stalls are garnished with

dogs' meat as well as with that of other animals. The farmers breed a certain race of dogs suitable for fattening, which they call market-dogs, (*chiens de boucherie;*) among these may be mentioned a variety of wolf-dog, with upright ears, remarkable in that the tongue, palate, and the whole interior of the fauces are of a black color. * * In certain restaurants of *our* large cities, cats are sometimes served up for rabbits; the Chinese use no such deceptions, considering cat's meat excellent, and at provision stores may be seen enormous cats suspended with their heads and tails. On all the farms, these animals may be seen, attached to light chains, undergoing fattening with the refuse rice that would be thrown away. The cats are large, resembling those found in our counting-rooms and parlors; the rest imposed on them facilitates the fattening process.

The rat also occupies a large place in the nutrition of the Chinese; it is eaten like the meats just described, either fresh or salted; the salted are chiefly destined for the junks. The farmers, seeing that this article is profitable, have even devised a plan by which they can reap some advantage from the fecundity of this animal. They have *Ratteries;* to establish these rat-lodges, they place in the corners of walls, that rats frequent, bottles with necks large enough to admit the hand; the animal, mistaking these bottles, fastened in the walls, for crevices, makes its nest in them, raises its young, and the farmer goes from time to time to remove the young rats, just as pigeons are removed from nests in pigeon-houses.

From rodents, let us pass to batrachians. In some countries the hind legs of frogs are eaten; the Chinese eat them entire, looking upon them only as we do on little birds. Still further, what is to be eaten must be decided by taste and not by appearance—a very rational Chinese proverb. The Chinese having tasted the toad and having found it good, have made it one of their ordinary articles of food, despite its repulsive appearance. In China not a single toad is wantonly destroyed.

One word as to the mode of preparing these meats so as to remove all feeling of repugnance. The animals, or quarters of animals, generally pass through the hands of the roasters, and these are the most celebrated in the world. They have fire-places so constructed that the fire is somewhat elevated; below, there is a support, to which are attached strings with hooks; these carry the article to be roasted, and the roaster from time to time twists the string above, so that by its untwisting and twisting the piece may be turned. The meats thus prepared are minced with a knife, and converted into hash, the form

in which they are generally eaten. A national sauce, called *sania*, is usually added, and rice serves for bread. On the tables of the rich, not less than thirty or forty such hashes are found, differing only in taste. One does not know what he eats. Dinner begins with preserved fruits, and rice has the honor of being the last mouthful.

The French use nearly all the shell-fish of salt water; the Chinese also eat those that live in fresh water and on land, such as muscles, &c. They have a species of monstrous snail—*voluto melo*—which is a favorite dish. * * The Australians devour the glutinous zoophytes which the sea throws up on their coasts; the Chinese employ them also, and, by aid of gastronomic proceedings, make good soups. In this class may be mentioned the *tre-pang*—Holothuria—which are dried and salted. They delight in everything that is gelatinous, mucilaginous and cartilaginous; sharks' fins, their swimming bladders, which they call *fish-stomachs*, the tendons of all animals, &c. All these are dried for purposes of alimentation.

Among the fish that are dried are some small ones caught by cormorants, which are employed by the Chinese fishermen. They are brought up with a ring soldered on the neck, which, being retracted, so closes the œsophagus that they can only swallow gelatinous materials. The fisherman carries several of the birds in the stern of his boat—setting them free on the sea, they plunge on the fish, seize and try to swallow it; but the ring preventing the passage of the fish, the bird returns to the boat to be freed from a body which is suffocating it, and the fisherman removes the fish.

In China there are some districts where the *arachnidæ* are a choice dish, and consequently the larvæ of insects—caterpillars of all kinds—are a common article of food. *One* deserves to be noticed—the silkworm; the Chinese hatch more than they have leaves of the mulberry, ailanthus, oak, &c., to feed until they arrive at maturity, and all that cannot be fed are cooked and eaten. And here is something still more strange. The chrysales of the cocoons are not lost; they are cooked, and form one of the prized aliments of the Mongolic race. A naval officer, who had eaten them, said lately that they were quite good, and compared a plate of chrysales to a plate of *maroons*. They also eat earth-worms, but only in times of great distress, considering them poor food.

Hatched eggs must not be omitted, nor confounded with putrid eggs; the former are eaten fresh, or preserved, cooked, and salted. They are preferred when they contain the young animal almost ready to break the shell. Their duck-boats are decorated with cages which

serve as a home for the ducks during the night, and with furnaces for hatching their eggs. As many as five thousand eggs have been hatched on some of these boats. The ducks are set free during the day, feeding on what is found in the rivers, returning to their floating home at night. A portion of the eggs thus hatched is destined for reproduction, and another portion is devoted to sale as public food.

A few words are required as to the birds' nests. These are nests of a swallow which frequents the seashore. It is found somewhat abundantly in the islands of Oceanica, Java, the Celebees, and the Molaccas, and along the Chinese coasts. Birds' nest soup is a luxury, if we may judge by its cost. Throughout China aphrodisaic virtues are attributed to it; in rich society, they endeavor to resuscitate passion by this food, which of course fails. These birds' nests have no other merit than that of containing about nine per cent. of nitrogen, which makes them nutritious and strengthening.

Many errors have prevailed as to the nature of this strange alimentary product. Some have said that the sparrow made it of a mastic that it prepared from the semen of the whale, obtained in the foam of the sea; others that it was formed from fish-spawn malaxated with saliva in its beak; others that it extracts this gelatinous substance from a species of Algæ or Lichen which it obtained on the rocks at ebb-tide. The truth is, that the nest is an immediate animal product of a peculiar kind—a species of mucus that the bird has the property of secreting in its beak, at the period of reproduction, in such quantity that it is able to construct its nest entirely of it. Payen calls this substance *cubilose*, from *cubile*, because it is produced by the sparrow for the preparation of a bed for its young. In studying carefully the internal structure of this white hemisphere glued to the rock, it is found composed of small filaments adhering together, and one is brought to the conclusion that the sparrow draws these out with its beak, like the silk-worm spins its cocoon. On account of this peculiarity in construction, the nest, (which is very hard,) when dissolved in water and converted into soup, still shows one portion in the form of a very fine vermicelli, while the remainder has dissolved as a jelly, and furnishes a soup of the clear brown color of strong beef soup.

The bird first constructs an external envelope out of small yellow roots, similar to those of the millet that are found in the sands of the seashore; then it forms the nest proper with its mucus, and it seems as it it drew out some of its feathers to decorate the interior, so that its eggs and little ones could rest on a soft and warm bed. These, like our own swallows, are fond of building their nests together; hence ag-

glomerations of six, eight or ten are often found so enlaced by their envelopes as to prevent separation.

In the crevices of rocks and cliffs these nests are built; sometimes caverns are found which are filled with nests accumulated for ages. A discovery of this kind is the discovery of a treasury. It is related of a wealthy Chinaman, who, after having been ruined, repaired his fortune by means of the discovery of a bird's nest cave, the contents of which were worth a million of francs. Indeed, the article is so prized that in the years when most abundant it is sold at 100 francs per kilogramme.

All the nests are not equally pure. Those that are perfectly white, after being cleansed, contain only the mucus of the bird. But when the bird has been disturbed in its first construction, either by enemies, such as birds of prey, serpents, or man, or by tempests, accidents, &c., it has no longer sufficient of its proper secretion to construct another nest, and its instinct teaches it to employ the mucus as a species of mortar to agglomerate other substances, such as algæ taken from rocky banks, and hence the cause of the belief that the nests were made from lichens. These impure nests are used also, but they are cheaper, and not in demand.

In making the soup, 120 grammes (corresponding in amount to one nest, or a nest and a half,) are put in a half litre of water, and boiled for two hours. It is very pleasant to the taste, although having a peculiar aroma. Soup for one man would cost in China 12 francs. In Paris they have been sold at one franc a gramme, 100 francs a kilogramme; and soup for one man would cost 120 francs.—*L'Union Médicale.*

L. H. S.

MEDICINE AND SURGERY IN CHINA.

BY M. G. PAUTHIER.

The practice of medicine and surgery is, in China, a very honorable profession, at the head of which is an Academy of Medicine, (*T'ai i youan,*) located at Pekin. The object of the latter is to maintain, in all its integrity, the science of medical practice, which dates from 3,000 years before our era, and to direct those entering upon such a career. The members composing this Academy are 115, 15 of whom are imperial physicians, (*Yu i,*) 30 practitioners, 40 doctors of medicine, and 30 aspirants. The imperial physicians are, in turn, on duty near the emperor and imperial family. They are often dispatched by the emperor to attend upon princes, princesses, ministers of State, and other great functionaries, when his majesty hears that they are sick.

Chinese medicine divides all diseases in *nine* great divisions, as follows: 1, Those affecting the pulse violently; 2, Those affecting it moderately; 3, Diseases produced by cold; 4, Diseases peculiar to women; 5, Cutaneous and painful diseases; 6, Diseases needing bleeding; 7, Diseases of the eye; 8, Diseases of the mouth and teeth; 9, Diseases of the bones. The physicians seem to have a tolerably good knowledge of anatomy, if we dare judge by the plates contained in their books. Their physiology rests on the system of two principles, *Yang* and *Ying*, or the *strong* and *weak* principle; the *male* and *female* principle, whose *equilibrium* and *harmony* constitute *the normal state*, and the predominance of either a *diseased* condition. Semeiology seems somewhat advanced with them; the practice of medicine having been, so to speak, hereditary in families, observation of diseases has produced the art of recognizing them, which has been pushed very far. I learn from several French missionaries, who have been treated by Chinese physicians in China, that these exhibited an extraordinary aptness in recognizing the external signs of disease. Observation of the movements of the pulse, on which they place four fingers of the hand, is carried much further than in Europe.

The practice of medicine is called "the benevolent art," *(jen chou,)* and is placed second to the profession of literature, which is the first. The following are the conditions exacted from those wishing to practice medicine: a *celebrated practitioner* must be sought out as teacher, to learn the principles of the science and the properties of remedies; the best authors that can be secured must be studied. Such works are common and numerous in China. When the student has accomplished the study of the best medical treatises, and has followed for a sufficient time the practice of his patron, he can practice himself. There are no public schools in the empire where medical students take their degrees and are publicly received as doctors; the great college at Pekin, and the academy alone, require examinations and confer diplomas on those who, desiring them, are recognized as worthy. The Chinese penal code provides for cases where ignorant men practice medicine with the sole idea of gain, and without the necessary knowledge.

"As for those who shall exercise medicine or surgery, (literally *internal* medical practice and *external* medical practice,) without understanding them, who shall administer drugs or operate with a piercing or cutting instrument contrary to practice and established rules, and, by such a course, shall have caused the death of the patient, the magistrates shall summon others of the profession to examine into the

nature of the remedy employed, or of the operation performed, which has been followed by the death of the patient. If it is manifest that they can be accused only of having acted through error, and without the intention of injuring, the physician or surgeon can be freed from the punishment inflicted on a homicide, in the way adopted in cases of accidental killing; but they will be obliged to give up the profession absolutely."

"If it shall appear that a physician or surgeon has intentionally not followed the established rules of practice, and while pretending to conquer the disease he really renders it more serious, so that the cure may bring him in more money, the sum which he shall have gained in this way will be regarded as stolen, and the punishment will be proportioned to the fees that he has received."

When a patient has died, and the physician or surgeon who has attended him, during the course of the disease, shall be convicted of having designedly employed injurious drugs, or of having done other injuries to his health designedly, he shall undergo decapitation, after having been retained in prison until the proper season."

The ordinary practice of medicine is principally based on very delicate and minute observation of the patient's pulse, on which curious treatises have been written. The rules of practice, recommended to Chinese physicians, are comprised in these four words: *wang, wen, wen, thsiei; examine, listen, question, feel.* These words are explained in this way: 1. *Examine* the countenance or physiognomy of the patient; 2. Notice the sound of his voice by *hearing* him speak; 3. Question him as to the origin or cause of the disease; 4. Feel his pulse.

The most common *surgical operations* among the Chinese are scarifications and acupuncture, which is done by needles figured in their surgical treatises. They treat fractures by means of bamboo splints, after having restored the fractured portions to their proper place. They also use internal medicines, which, they say, possess the property of uniting bones.

As for the resources of the general therapeutics of China, in cities where Europeans reside, as Macao, Canton, Shanghai, Ningpo, Amoy, Hong Kong, these are nearly the same as in Europe, with some Chinese medicines added. The Chinese *materia medica* embraces, in some way or other, all creation. We learn from a treatise on this subject by *Li-chi-tchan*, in 40 vols., "all things produced in the world, birds, quadrupeds, insects and fishes, have power over the breath and the circulating blood; the same is true of flowers and trees, which, although possessed of life, are not supplied with either breath or venous blood;

inanimate objects also, such as stones and metals, may be employed as therapeutic agents." The objects examined, in the Chinese book in question, are comprised in 1,871 genera or classes.

All the medicines employed by the Chinese are not prepared by licensed pharmaceutists, but by those who have acquired knowledge in this kind of business—the herborists—who sometimes hawk their merchandise about.—*L'Union Médicale.* L. H. S.

DALTONISM NOT CONGENITAL.

BY DR. CLEMENS, OF FRANKFORT.

It is known that many persons are affected congenitally with blindness to all colors, or to certain of these, or rather that many persons receive from certain colors an impression differing from that ordinarily produced. Cases, in which this alteration is *acquired*, are comparatively rare, and hence the following is of interest.

A lady, thirty-two years of age, always enjoying good health, the mother of three children and then in the fifth month of her fourth pregnancy, was walking with her friends. They met a woman wearing a flashy red shawl, which attracted attention; the lady making no remark at it, her opinion was asked, and she answered that she saw nothing extraordinary, as the shawl was of a gray-mixed color. This was probably the beginning of the disease, since the husband stated that some days afterwards his wife, in buying goods, rejected some because they contained red and yellow. About the middle of her first pregnancy an analogous condition existed for a short time. She was working at a piece of embroidery requiring many colors, and, on different occasions, the colors seemed, as it were, to run with each other at their lines of contact, and when an effort was made to determine the terminal lines of the colors, she saw a mixed gray, and a nausea supervened, almost producing vomiting. Her eyes appeared a little dissimilar—the left being somewhat more prominent than the right; the pupils were slightly dilated, and not very sensible to light. All the functions were normal. None of her relatives have been affected with this alteration of vision. The patient often confounds red and green in a peculiar manner. Thus, if the two colors are shown at the same time, they are confounded the one with the other, while the red being shown by itself, is never taken for green. Flashy red objects, seen alone, appear of a dirty grayish-brown. The loss of perception of red is in no instance more marked than when a pure red is shown on a white background; for example, carmine on a porcelain saucer. All that remains of the color is its skeleton; that is, the shade.

During ten days the phenomena seemed to increase; red and green became more confounded, and the shades of other colors were only recognized by careful attention. Thus she confounded yellow colors with orange, and blue with light red tints.

Dr. Clemens follows this report with physiological and pathological considerations in this disease of vision and other analogues. Passing in review the different theories which have been advanced on the subject, he shows their unsoundness; and attaches himself to the theory of Schapenhauer, which does not recognize colors as having an existence in themselves, but as products of the retina.—*Gazette des Hôpitaux.* L. H. S.

SANTONINE—ITS EFFECTS ON VISION AND THE URINE.

FROM THE FRENCH, FOR THE MONTHLY.

Mons. de Martini notices that the employment of santonine is followed by two distinct effects—a coloration of vision and of the urine; and of these, the former is much the more curious. Of those who have taken santonine, most see objects colored green, some blue, and others a pale yellow. The following singular case is narrated: A woman, seventy years of age, had experienced for some time a weakness of sight in the left eye. The exterior of the eye exhibited no alteration; the pupil was slightly sensible to light, and larger than that of the right eye—in the aqueous humor a slight white mist was perceptible. She was scarcely able to distinguish light. Santonine was administered, 4 to 6 grains daily, beginning on the 10th of March; on the 15th the patient saw, four or five times during the day, objects colored greenish-yellow, even with the affected eye. On the 18th eight grains of santonine were given, and in addition to the fact that objects were colored as before, the patient began to recognize the forms of the assistants. On the 20th and 22d of March, she saw objects colored yellow, but continued to recognize them still better. The administration of santonine ceasing, the improvement continued.

L. H. S.

THE VALUE OF SULPHATE OF CINCHONIA IN INTERMITTENT FEVER.

BY DR. MOUTARD-MARTIN.

The author of this memoir presents the following conclusions:

1. Sulphate of cinchonia has an incontestable, although variable, action in intermittent fevers.

2. Sometimes its action is rapid, and it cuts off the attack like sulphate of quinia; at other times it is slow, whatever be the dose, and the paroxysms are gradually checked.

3. The dose should always be larger, at least one-third more than that of sulphate of quinia under the same circumstances.

4. To obtain its curative action, the dose will vary from nine to fifteen grains; at this dose it produces frequently some physiological effects, which it would not be prudent to exceed.

5. Its therapeutic is not proportional to its physiological action, since it cures at times without any perception of the latter; and in other cases where the physiological action is energetic, the therapeutic action fails.

6. It cannot replace sulphate of quinia in the treatment of severe intermittent fever, but may be considered an important adjuvant to the latter, completing the cure begun by the same.—*L'Union Médicale.* L. H. S.

MONTHLY SUMMARY OF MEDICAL JOURNALISM.

By O. C. GIBBS, M.D., Frewsburg, N. Y.

Bismuth in the Treatment of Burns.—In the *N. A. Medico-Chirurgical Review*, for July, Prof. T. G. Richardson has an article on the treatment of burns and scalds with the subnitrate of bismuth. He says he has previously been in the habit of using the white-lead and linseed oil, as first recommended by Prof. S. D. Gross. Though he considers this treatment with lead superior to all other applications previously recommended, he says, "I am now convinced by ample experience that the bismuth is better." * * * "I was induced to give it a trial from a consideration of its well-known effect in calming irritation, and even actual inflammation occurring in mucous membranes, the condition of these structures under such circumstances bearing a very close analogy to that of the skin after a burn of the first or second degree. When I first began its use, I combined it with linseed oil in such proportions as to form a consistent paint, but subsequently substituted glycerine for the oil, and I am now inclined to think that the combination can never be surpassed, since by it every local indication is fully met." To prepare it, it is only necessary to rub the bismuth in a mortar with a sufficient amount of glycerine to form a paste or thick paint, which should be applied to the affected surface by means of a camel's-hair pencil, or a mop made of soft linen." * * "In burns of the first degree, one such application will often suffice; but in those of the second degree, it may be necessary to repeat it, in

part at least, from day to day, in consequence of its disturbance and the wetting of the cotton by the subjacent discharges."

We have neglected to mention that, after the application of the bismuth and glycerine, Prof. Richardson advises that the parts be covered "with a sheet of clean carded cotton, or a layer of cotton batting, which may be confined, if necessary, by a thin bandage lightly applied." He says, "The carded cotton or cotton batting I look upon as a most valuable adjuvant, and is superior to anything with which I am acquainted for warding off pressure."

Dr. W. C. Nichols, House Surgeon of the Charity Hospital, New Orleans, upon the recommendation of Dr. Richardson, has put the bismuth and glycerine to the test of experience, and "freely accords to it all the praise which Prof. Richardson bestows upon it."

Opium in Pneumonia.—In the Report of cases of Pneumonia, treated at the Charity Hospital, N. O., under the charge of Prof. Austin Flint, and by him reported for the *New Orleans Medical News and Hospital Gazette*, for July, the following language occurs in regard to the use of opium in pneumonia: "Of the value of opium in the treatment of pneumonia, my experience has furnished abundant evidence during the last five or six years; and the more my experience accumulates, the more am I satisfied that the free use of this drug will come to be regarded as a very important measure in the management of certain cases of the disease."

Our experience is in full conformity with the above, and we are happy to adduce so high an authority in support of our opinions, long since entertained and expressed. Though most authoritative writers condemn the use of opium in pneumonia, we were early induced to depart from standard authorities, and each year's experience but adds to our conviction of its propriety, and to our confidence in its utility. Our own views are given in the MONTHLY for January last, in a notice of W. W. Gerhard's work upon Diseases of the Chest.

Treatment of Yellow Fever.—In the *New Orleans Medical News and Hospital Gazette*, for July, Dr. F. Poland, of Houston, Texas, has an article upon the treatment of yellow fever. He has derived favorable results from the use of nitro-muriatic acid and chlorine. Even after the occurrence of black vomit, he says the medicine often acts charmingly. Chlorine is by no means a new remedy—it forms a part of the compound so successful in the hands of Prof. Fenner and others, who have put it to the test. The prescription used by Dr. Fenner we gave in our *Summary* for June. The following is the formula of Dr. Poland:

℞.—" Acid. nitro-muriatici,
Liq. chlorini, ää, ℥ij.
Syr. aurant., ℥j.
Aquæ dist. q. s. mist., ℥vj.

S.—Table-spoonful every two or three hours." This he uses in all stages of the disease.

Delivery of the Placenta in Abortion in Early Pregnancy.—In the *Lancet and Observer*, for July, Dr. L. D. Sheets has an article upon the above subject.

It is a generally acknowledged fact that most of the alarming cases of hæmorrhage, accompanying early abortion, are caused by a *partially* detached placenta. In such cases the complete detachment and removal of the same constitutes the appropriate treatment. The important inquiry is, how can this best be done? But to the paper. In regard to diagnosis, Dr. Sheets says, "We make out the diagnosis by the *touch*, as we can often, without any extra effort, feel part of the after-birth at the internal os; but if this examination prove unsatisfactory—if we feel nothing—we must introduce the finger still further, *until we can traverse the whole internal surface of the womb*, and in perhaps every case we will find a placenta." This may be a feat of easy performance, but we have seen cases in which we could not perform it, and we have the satisfaction of knowing that some of our ablest and most experienced obstetricians have been placed in the same unfortunate condition. Hence, it may be well for the young practitioner to be prepared for a failure in this regard, and it may not be amiss to be acquainted with other resources.

We give Dr. Sheets' method of removing the placenta in these cases, which removal is really the object ever to be had in view. "My method is to remove the placenta with the index finger, on which I always keep a tolerably long nail, for the purpose of separating the after-birth from the womb. The nail should not be too long, or it will not be sufficiently firm, and may bend backward while using it. Push the uterus as low down in the pelvis as possible, and retain it there with the left hand, while you introduce the index finger of the right hand into its cavity, and pull off the placenta, and remove it. Sometimes it is a little difficult to separate with a single finger, and I do not get quite all removed; but it is so much broken up as to arrest the violence of the hæmorrhage, and render the tampon, &c., unnecessary; in a few days the remainder will come away piecemeal."

So it seems that Dr. Sheets is not always quite successful in the removal. We have a horror of "piecemeals," and believe tha

cess may always crown our efforts. Because of the vast importance of the subject, our readers will pardon a few comments, if even a little critical. Dr. Sheets may always, by this means, arrest the hæmorrhage; but we know that we cannot, and we fear there may be some that might be alike unfortunate, and, hence, for us, there must be found another and easier road to success.

We have seen several cases in which it was all we could do, with justifiable force, to touch the os, saying nothing of "traversing the whole interior of the womb."

Prof. Meigs is free to confess that there are a good many cases of "abortion in the early stages," in which he has failed to extract the placenta. Of this acknowledgment Dr. Sheets, says, "How remarkable that the corypheus of obstetricians in this country should fail in what *can be performed in every instance!*"

Has he forgotten that he sometimes succeeded only in part? When Dr. Sheets has seen, as we have, a girl of 14 years, about at two and a half months, with a vagina undisturbed by previous childbearing, and, because of youth, at best but imperfectly developed, and the os uteri only just within reach, he may find a case of failure, unless he has a very small hand, or has a finger-nail two inches in length. Ramsbotham and others have called attention to the fact that there are some cases in which the hand cannot be introduced into the vagina. Dr. Sheets thinks it strange that it should not occur to so sagacious authors that if the hand cannot, the finger can. We repeat, that we have had cases in which the finger was not long enough. We suppose that others may be as unfortunate. Dr. Sheet says, "On account of having *short fingers*, I am often obliged to introduce my hand into the vagina; but I *always reach the fundus of the womb without difficulty.*" We repeat, there are cases where a physician would not be justified in using the requisite force for the introduction of an ordinary-sized hand, at least so long as there is a better, easier, more expeditious, and less painful way to accomplish the same end. Bearing upon this point, we quote, with pleasure, from Dr. A. K. Gardner, of New York. He says, "How shall the small and fragile placenta be seized hold of and withdrawn? Some have recommended the introduction of one finger into the uterus, and bringing down one edge of the placenta, and twisting it round and round, not only thus to detach the entire mass, but to also so shape it that it may the more easily pass through the os. Where this can be done, by all means do so! But it should be remembered that in the great mass of cases *it is impossible to reach the os, so as to pass one finger into the cavity;* far less to

effect any good result, if it arrive there, to say nothing of the utter impossibility of aiding the finger with the thumb of the same hand." Thus it will be seen that we are not alone in finding cases in which the unaided finger is inadequate to the successful treatment.

Dr. Sheets has a poor opinion of the tampon in these cases, believing them to be productive of much mischief. In this opinion we concur most heartily, but we cannot join him in the condemnation of instrumental aids in the delivery of the placenta. We have seen that, however successful Dr. Sheets may be in making the unaided finger accomplish all that is desired, there are those, and we among them, who are very far from being so successful.

We have been in the habit, for some time past, of delivering the placenta, in these cases, with Carey's *decidual separator*, if the finger is inadequate to the end. With this the separation is easily made, the evulsion speedily accomplished, and the consequent hæmorrhage ceases at once and permanently. The irritation from the introduction of the instrument is far less than from the introduction of the hand, especially when the abortion is in a primipara, and at a stage anterior to the fourth month. Our rule is to lose no time with the trial of styptics, the tampon, &c., but to separate the placenta, and remove the secundines at once, *with the finger if we can, and with instruments if we must.* At all events, effect the removal, and the hæmorrhage ceases, and all danger is past.

Uterine Neuralgia.—In the *Lancet and Observer* for July, Dr. E. J. Fountain reports a case of severe uterine neuralgia, which resisted the usual treatment; large and repeated doses of opium included. At this stage he says: "Recollecting an account of a case of facial neuralgia promptly arrested by the muriate of ammonia, published in *Braithwaite's Retrospect*, I concluded to give it a trial in this case. I ordered half a drachm to be taken every hour, until some effect should be produced. She began to feel some relief shortly after the first dose, still more after the second, and the third dose removed every vestige of pain, and she dropped into a natural and refreshing sleep, the first she had enjoyed for ten or twelve days. She continued taking the preparation for several days, three times each day, in gradually diminishing doses, and had not the slightest return of the difficulty."

Anti-Lactescent Properties of Belladonna.—Dr. Fountain, in the same number of the *Lancet and Observer* just referred to, reports a case of a lady whose breasts had continued to secrete milk without cessation for three years. Various remedies had been brought to bear upon

the case, and persistent effort made to arrest the secretion, without effect.

Dr. Fountain ordered the following:

℞.—" Ext. belladonnæ,
Ung. glycerini, ää ʒij. M.

To be rubbed freely over *all* the breast, and covered with oiled silk to protect the clothing. By the time this was all used, less than a week, the secretion of milk was entirely arrested, and has not since returned." In our hands the belladonna has so often and speedily arrested the secretion of milk, and thus abated mammary inflammation and prevented abscess, that we regard it as much a specific in these cases, when timely and properly used, as is quinine in ague. We have used Tilden's fluid extract, without admixture.

Artificial Respiration for the Cure of Opium Poisoning.—In the *Chicago Medical Journal* for July, Dr. A. Groesbeck reports a case in which a fluid ounce of laudanum was taken, and life preserved by the *ready method* of Marshall Hall. An hour after the laudanum was taken the stomach was emptied by means of a pump. A little later " he sank into a profound coma; his pulse scarcely perceptible; skin cold; face turgid and livid, with, at times, hardly a sign of life." Artificial respiration was now resorted to, by the method proposed by Dr. Hall, and was kept up for nearly six hours before signs of improvement commenced. From this point improvement was rapid, and recovery was after a little complete.

This is not new treatment, but it is additional evidence of the utility of artificial respiration for the relief of opium poisoning.

Quinine as a Prophylactic of Malarious Diseases.—In the *Charleston Medical Journal and Review*, for July, Dr. H. W. De Saussure has an article on the preventive power of quinine over intermittent and remittent fevers. Many facts giving evidence of such power are given, but our space will not admit of recapitulation. We instance only one or two. In 1840–'41 the English government sent two well-manned vessels to explore the river Niger. Of the two ships' crews, all were sick with malarious diseases, the expedition was abandoned, and but very few returned alive. In 1854–'55 the effort was renewed. From the time of entering the river, until the time of leaving, a period of two years, five grains of quinine were taken in the morning by every man in the expedition, and but very few suffered from disease—the expedition accomplished its object, and returned without the loss of a man with malarious disease, and the crew presented every evidence of robust health. We quote one circumstance that came under Dr. De

Saussure's observation. "I was called in August to see one of the contractors on the Charleston and Savannah Railroad, laboring under a very severe attack of remittent fever, contracted during superintendence of his contract between the Ashepoo and Combahee Rivers, notoriously a very unhealthy region. During his convalescence, he informed me that he had a large number of hands employed (150); that they were negroes brought from healthy regions in North Carolina, and he expected all of them to be more or less sick, as they were entirely unaccustomed to a malarious climate. I advised him to take quinine daily himself, and to give it to all his hands, white or black. Late in the fall, I met him in the city; he looked healthy and well. He thanked me for the advice I had given him; told me he had carried up some pounds of quinine; had used it himself daily, and compelled all his employees to take it also; that he himself had never had another attack of fever, that his health was better than it had ever been, and that not a single one of the 150 hands he employed had been attacked by fever."

For those who have never doubted the prophylactic powers of quinine over malarious diseases, the cases referred to in the paper under consideration are quite conclusive. Still there are some who consider quinine a poison, and detrimental to any constitution long under its influence. It will be remembered, however, that the men in the Niger River Expedition took the quinine daily for two years, and yet each presented the appearance of robust health. In all the cases referred to, quinine seemed to augment the conditions of health rather than to produce any injurious consequences upon the constitution.

Epilepsy.—In the *Charleston Medical Journal and Review*, for July, Dr. W. M. Cornell has an article on the treatment of epilepsy. Dr. Cornell has given considerable attention to this disease, and has previously published essays upon the subject, giving expression to his opinions.

The following formula has given good results in his hands, in suitable cases:

R.—"Spiritus vini gallici,		Oj.
Tinct. stramonii dat.,		℥jv.
Sulph. Zinci,		ʒij.
Solv. zinc. in aquam distil.,		℥j.
Add sol. tincturæ.		

S.—Commence with gtt. No. 10; increase by one gtt. till the specific effects of the narcotic appear, then keep as near that maximum dose as the circumstances indicate." * * In epilepsy, caused

by *self-abuse*, Dr. Cornell speaks highly of oil of cloves, which he regards "as worthy of trial in all cases of this kind."

General Bloodletting.—In the *Atlanta Medical and Surgical Journal*, for July, Dr. Robert Southgate has a well-written article of 42 pages upon the subject of bloodletting in the treatment of disease. The doctor leans "to the opinion, that we are making a retrograde movement in the growing tendency to give up general bleeding in the treatment of disease," and says "if the medical history of the past teaches any truth in reference to the future, there is a destined reaction to take place in medical opinion. Another petechial fever may break out, in which indirect will be mistaken for direct debility. Another Rasori, captivated by the simplicity of the doctrine, will resort to stimulation, and be awe-stricken by the frightful mortality. Is it not the duty of each one of us to give this subject a thoughtful reconsideration?" While he admits that, during the last half century, there has been a great and wholesome revolution effected in the science and art of medicine, he thinks that revolution is taking quite too headlong a course, and says, "I verily believe that some of our most valuable therapeutic means are falling into unmerited disrepute; and that the future improvement of practical medicine will depend upon a careful review of them all—their powers for good and evil, and their peculiar adaptation, under the cautious guidance of reason, judgment, and common sense, to the relief of the various morbid conditions we are called upon to treat."

Dr. Southgate's opinions are worthy of respect, for, having been long in public service, he has observed disease in diversified localities, "from the great lakes of the North, to the San Saba River in Texas; and from regions far beyond the Mississippi, to the shores of the Atlantic." Space will not permit a full synopsis of the opinions and arguments of Dr. Southgate; yet, in the fulfillment of our purpose to make our *Summary* a complete index to all important practical original papers to be found in the current medical journal literature, we cannot pass by the one under consideration without briefly indicating its contents. Dr. Southgate commences his application of general bloodletting to the treatment of disease, with *pneumonia*. He is an advocate of bloodletting in the early stages of this disease, believing that it is a powerful remedy for the prevention of the exudation of lymph, which often fills the air-cells and interferes largely with the aeration of the blood. Our readers may think we stand committed to an opposite opinion; but this is not so. We have never denied the utility of bloodletting in pneumonia—all we have attempted is to prove

that *all* cases do not *require* it. In fact, we have not seen a case that did for the last three years, but how soon we may is quite uncertain. As pneumonia has presented itself to us, for the three last years, quinine and opium have been admirably adapted; but we are confident that to treat all cases, in all places, thus, would be an error only surpassed by the more common one of bleeding in all cases. But we are digressing from the paper. Dr. Southgate says, "We all know that many cases of pneumonia will recover without bloodletting, or other active treatment; that rest in bed, the removal of all sources of irritation, warm poultices to the chest, mild diaphoretic and demulcent drinks, will, in time, effect a perfect restoration. But if from this we are in danger of arriving at a generalization, false—and if false, fatal—that general bloodletting is never necessary in pneumonia, is it not our duty to pause and reflect whether the current with which the medical mind is now drifting will carry it to a safe anchorage; or whether our bark, freighted with such precious interests, is not being wafted towards shoals and rocks and deceitful quicksands, on which it may experience a disastrous shipwreck?"

Dr. Southgate says he is satisfied, from a reasonable experience, of the utility of general bloodletting in the cold stage of *malarial fever*. It seems to us that cases requiring such treatment must be very few. When the patient is first seen in the chill, and that chill is of a congestive character, calculated to do injury, perhaps general bloodletting might be of service, as a temporary means, while waiting for the period, anterior to the next paroxysm, in which quinine can be best administered. *Our* remedy here, unless contra-indicated by special symptoms, is a large dose of opium, which will greatly lessen the duration of the hot stage.

In *continued fevers*, except of the typhoid type, Dr. Southgate believes that bleeding is too much neglected. Malignant cases, he believes, are those where the powers of nature are "oppressed and overcome by the first attack in the disease, so as not to be able to raise regular symptoms, adequate to the violence of the fever—all appearances being quite irregular." * * "I remember to have met with an instance of this kind, several years ago, in a young man I then attended; for though he seemed, in a manner, expiring, the outward parts felt so cool that I could not persuade the attendants he had a fever, which could not disengage and show itself clearly, because the vessels were so full as to obstruct the motion of the blood. However, I said they would soon see the fever rise high enough upon bleeding him. Accordingly, after taking away a large quantity of blood, as

violent a fever appeared as I ever met with, and did not go off till bleeding had been used three or four times."

Dr. Southgate would recommend general bloodletting in many other diseases, but we have not space to specify. We will conclude our remarks upon this paper by referring to his opinions of bloodletting in pregnancy. Our mothers and grandmothers, who were often bled during gestation, he thinks went through with the perils of maternity with but little detriment to health, preserving their health and vigor until mature years; while, in these days, scarcely is the young wife the second time a mother, when she presents too frequently a wreck of her former self. Intra-pelvic irritations and pains harass her by day; congestions, inflammations, and ulcerations of the cervix uteri, and their sympathetic accompaniments, destroy her bloom; and we hear doctors boasting of how often they have leeched and cauterized the delicate structures." In regard to this subject he concludes by saying, "Now, may it not be possible that much of this distressing ill-health is due to the fact, that we let the sufferer go through nine long months, with her headaches and flushings, her febrile movements, her intra-pelvic pains, her gastric irritations, her bloated limbs, and the other afflictions of her most interesting state, when experience has proved that the occasional loss of a few ounces of blood from the arm would soothe her troubled system into peace and comfort."

With these quotations and remarks we leave the paper of Dr. Southgate, reminding our readers that it is one of the ablest advocates of general bloodletting, in inflammatory diseases, that has fallen under our observation. We are certainly in favor of a reconsideration of all the evidence in favor of and against bloodletting in inflammatory diseases. Because we have regarded this subject of the first importance, have we given the above space to its consideration. We shall recur to it on a future and suitable occasion. The paper of Dr. Southgate appears also in the *Southern Medical and Surgical Journal*, for July, and we understand the author has also issued it in pamphlet form.

Quinine in Cholera Infantum.—In the *Oglethorpe Medical and Surgical Journal*, for May, Dr. J. S. Rich, of Florida, has an article upon the treatment of cholera infantum. He says he has seen much of the disease, but his success was not remarkable until, in 1855, he commenced treating with large doses of quinine. Among others, he reports the case of his own child. We quote his treatment; the following was ordered:

℞.—" Calomel,	grs. iij.		
Sul. quinine,	grs. v.		
Oil tereb.,	gutt. xx.		
Honey,	℥j.	Mix.	S.,

given at one dose. In the course of some twelve hours this dose was repeated, (omitting the calomel.) Under the influence of these doses, for the period of some thirty-six hours, I could not detect any other effect of the quinine, than a most profound and salutary sleep; the pulse, which had been much too frequent, irregular, thread-like, and fluctuating, became slow, as in health, and firm; the skin was continually moist with warm perspiration; the kidneys acted most copiously; bowels acted twice, the last showing the liver was performing its healthy functions. The most remarkable change was the disappearance of the general cadaverous aspect and *grave-yard odor*. The return of appetite and general powers of digestion was extraordinary."

Vaccination.—In the *Maryland and Virginia Medical Journal*, for July, Dr. Blair Burwell has an able article upon the subject of *Varioloid*, and the protective power of vaccination. Dr. Burwell is a believer in the temporary protective power of vaccination, and adduces in proof that "the authors of all the memoirs presented to the French Academy in 1845, unanimously agreed that, with very few exceptions, the small-pox attacks those who have been vaccinated for a long period of time, and spares those who had been recently so." In regard to the duration of the protection he says: "In the cases of varioloid and small-pox which I have observed, they have never occurred until after the tenth year from vaccination, and the revaccinations which I have performed have invariably been unsuccessful previous to the seventh or eighth year from the first vaccination, and after the tenth year a large number have been successful, with a good many cases partially successful, and *very, very few* in which no impression has been produced." He thinks protective influence lasts from ten to fifteen years; and in regard to revaccination he says, "May we not be safe in recommending a regular time for renewal, midway between these two periods—say twelve to thirteen years, unless epidemic disease should require it oftener, and then we might adopt ten years."

Our space will not permit us to follow Dr. Burwell through his paper; we subjoin his concluding paragraph: "I shall continue to believe that the protective power of vaccination is temporary; that revaccination does add or renew security against the small-pox and varioloid; that the period of time for which it exerts its protection is between ten and fifteen years; that it ought to be renewed regularly between those periods; that the vaccine virus of the present day has degenerated, or become weaker in its protective powers; and, lastly, that a resort to the original source is the proper means to restore it."

Upon this subject we have, for many years, entertained different

opinions from those expressed by Dr. Burwell, at least in some particulars. If Dr. Burwell's facts are reliable, he supports his opinions with a strong array of arguments. We have not space to enter here into the discussion of the subject; but will simply suggest that, if the influence of vaccination deteriorates with time, varioloid should be more and more common and more and more severe, as the period from vaccination lengthens, *until the protection should be entirely null, and the patient should be as liable to take small-pox as before the vaccination!* Now this is not the fact. We have seen varioloid in a person 50 years after vaccination, and in a child not three months after the manifestation of the cow-pox, yet have not witnessed any marked intensity in the former case. The returns from the Army and Navy of the United Provinces of Great Britain are still more conclusive upon this point. Out of 43,800 soldiers, under 20 years of age, there were 15 deaths from varioloid; while with the increase of age and the lengthening of the interval from the period of vaccination the number of deaths gradually decreased, until out of 34,300, over 34 years of age, there was but one death from varioloid. We have studied this subject with considerable care, and consider that, so far as we are able to appreciate the evidence, the facts are in favor of the conclusion, that *protection is as good at 50 as at four years of age.* We confess that from 15 to 25 years of age is the period in which the most cases of varioloid make their appearance; but, as this liability to varioloid diminishes from this time on through the remainder of life, this increased susceptibility must depend upon some constitutional causes, *and not upon a diminution of the vaccine protection.* From all the facts we have been able to gather upon this subject, *we believe that a genuine and successful vaccination is a sure protection against small-pox, and, in the majority of cases, against varioloid also;* and this protection, so far as it goes, we believe to be as good at 50 years as 5 months after vaccination. In a minority of instances the susceptibility is not fully exhausted, and the patient is subject to varioloid. *By revaccination, if genuine and successful, the protection is rendered as complete as possible, and the patient, except in a very few rare instances, is no longer subject to varioloid.*

It is of but little or no consequence whether the revaccination is made 12 months or 12 years after the first vaccination. Upon this point Prof. Wood says, "I have been in the habit of vaccinating the second time for 27 years, and during that time have seen *no* case of varioloid in any one that I have revaccinated." So long, however, as there is upon this point a difference of professional opinion, and be-

cause revaccination is neither painful or otherwise objectionable, we see no impropriety, but rather an increased security, in a revaccination every 10 or 12 years. We, however, object to and deprecate any attempt to lessen public confidence in the protective powers of vaccination. There are already quite too many that neglect to avail themselves of its unmistakable advantages.

Diet for Dyspeptics.—In the *Maryland and Virginia Medical Journal* for July, Dr. A. B. Tucker has a very able article upon *dietetics*. His remarks upon the diet appropriate for the consumptive patient are very appropriate, but too lengthy to be copied here. We quote a passage in regard to the diet appropriate for the dyspeptic: "There are a variety of breads which have been offered to dyspeptics as certain remedies, from that made mechanically to that which acts mechanically; bran bread, rye bread, &c., have been supposed to exert a good influence in dyspepsia, by keeping the bowels open by their mechanical effect of irritation; an object very desirable if it can be attained, but I doubt very much the propriety of giving indigestible food to a stomach already weakened, and whose complaint is, it cannot digest."

The above idea is in full accordance with our own opinions. The idea of giving indigestible bran, &c, to a patient is quite absurd. We have, in several instances, seen indigestion perpetuated, and the patient waste as in consumption, by an injudicious dieting with indigestible substances. We well remember a case in which a bread, made by a mixtnre of wheat bran and quick-lime, wet up with cold water and baked, with no addition of yeast or other substance, formed the principal diet! Dr. Tucker says further: "Does it not seem plausible, that the constipation is due rather to the disease of the stomach, than to any lesion of the bowels, which could be overcome by mechanical irritation? I believe cold wheat bread is the most digestible, and therefore the best. I advise it to be cold and stale to some degree." It is evident that bran and other mechanical irritants, though they may relieve the constipation, are not remedial, and are only a present relief of one symptom, purchased at the risk of aggravating the real difficulty. We shall make but one other quotation from Dr. Tucker's paper: "No meat should be used," (by the dyspeptic,) "for the reason that the weak stomach would find difficulty in digesting it, and because the constituents of it can be introduced better in other forms." We do not fully subscribe to the above. We believe it is easier for an enfeebled stomach to manufacture healthy blood out of meats, than vegetables; out of good beef, than out of cabbage, potatoes, or even

bread. Extract of lean meat, concentrated beef-tea, beef cut as for sausages, and very lightly cooked, or oysters, each properly served up, are unobjectionable, as a part of the dyspeptic's diet.

A Severe Burn Treated with Alum.—In the July number of the *Maryland and Virginia Medical Journal*, Dr. Wm. M. Turner reports a case of severe burn, that was unexpectedly cured by a new method of treatment. The burn was in a person of broken-down constitution, and of intemperate habits, and was very large in extent. The Dr. saw the case first a week after the accident; he says, "A more foul and offensive wound I never saw before in the wards of any hospital. The odor was almost unbearable. The man was quivering with pain, wore an anxious look, and had a small, thready, irritable pulse."

Treatment was instituted, and continued for a considerable time, with no favorable influence. The patient continued, apparently, to sink under the exhausting influence of the offensive discharges, and occasional profuse hæmorrhages. For the purpose of arresting the hæmorrhage, powdered alum was applied, and the general influence was so good as to induce its further continuance. He says, "I continued with the alum powder, dusting the entire surface, and instead of covering with rags spread with simple cerate, I applied the cerate of the impure carbonate of zinc, or calamine cerate. This I did not spread on a cloth, but melted in a large iron spoon, and after the alum was well applied, I poured the melted cerate over the entire surface, and covered the wound with very light oil silk. From that moment I had no more trouble; the man slept so well that anodynes were dispensed with, and nature afforded rest, unaided. The tonic treatment soon produced an appetite, and rich food gave the patient good blood. The wounds healed very rapidly after commencing the alum treatment; and what is more singular, more fortunate, and very inexplicable, there was no *drawing cicatrix left*. The man is to-day in better health than he ever was, and can walk anywhere he pleases."

A New Operation for Amputation of the Foot.—In the same number of the above-mentioned Journal, Dr. A. P. Smith recommends a new operation for amputation of the foot. Instead of disarticulating, as is usual in all the various plans hitherto practiced, Dr. Smith advises a direct section of the bones. This operation is easier to perform than one that requires the knife to follow the irregular tarso-metatarsal articulations, exposes much less bone surface, and gives a granulating surface of bone rather than a cartilaginous, which is an acknowledged advantage. It is further claimed that "the granulating ends of the bones more readily form union with the surrounding

soft parts," and "the preservation of the articulations renders the result more satisfactory, and less limping and discomfort in walking."

Orchitis.—In the *Pacific Medical and Surgical Journal*, for June, Dr. E. P. Vollum has an article upon the treatment of acute orchitis. Dr. Vollum speaks highly of *tartar-emetic* in these cases, and he thinks it is preferable to any other antiphlogistic, as general bleeding, mercurials, opiates, &c. He says, "In tartar-emetic we have an agent all powerful to control the erotic erethism, and the attendant flow of blood into the congested or inflamed testicle." It is to this property that he mainly ascribes its beneficial influence. He also, because of this influence of the medicine, thinks its use is indicated in cases of enlarged prostate, cystitis, gonorrhœa, and in many pelvic affections of females.

Menorrhagia Treated with Cream of Tartar.—About twelve years ago, a writer, in some of the European journals, spoke in high terms of the *bi-tartrate of potassa* in the treatment of menorrhagia. This remedy in this affection has received but little attention at the hands of the profession, and certainly has not secured professional confidence in its powers over this disease. In the *St. Joseph Journal of Medicine and Surgery*, for July, Dr. J. A. Chambers has an article upon the subject. He says he has used the remedy for the last twelve years, in this disease, with greater success than has followed any other remedy in menorrhagia. He directs his patients to "take three teaspoonfuls of the medicine and put it in a sufficient quantity of boiling water to dissolve it, adding sugar sufficient to make it palatable, to let it cool, and drink the quantity in twenty-four hours; when that is gone, to use more in the same way."

Constipation Treated with Colchicum.—In the *Journal of Materia Medica*, for July, Dr. Joseph Bates has an article upon colchicum. Among other affections in which he speaks well of it, he mentions constipation, in which, he says, it has seldom disappointed him. "For constipation, I give eight drops (of the tincture) every four hours, and continue its use a few days, adding or diminishing the dose as circumstances may indicate."

Lupus.—In the *Boston Medical and Surgical Journal* for July 5th, Dr. J. C. White has an article upon lupus, which shows, on the part of the author, a thorough acquaintance with the subject. Upon the treatment his remarks are full; we quote the most important and more novel: "But of all remedies for lupus, the anhydrous or stick nitrate of silver is incontestably the best, and the best in every case. It can be trusted in the hand of any one, however inexperienced in the treat-

ment of the disease, and cannot possibly do harm, because it is held in complete control, and because the sound tissues are very little, if at all, affected by its contact, while the diseased parts may be thoroughly pierced and penetrated to their very bottom. It is not enough, however, as is often done, to apply it to the surface merely, but a sharply-pointed stick of the material, set firmly in a quill, must be taken, and thrust boldly down to the limits of its penetration. In the beginning of the treatment it is well to apply the caustic thus several times, at intervals of three or four days, till we obtain a smooth, even, suppurating surface. Arrived at this point, the process must be repeated twice a week, after which once will be sufficient. The scars which result from this treatment are the fairest and finest of scars."

Dr. White does not claim this as an original plan with himself. It was perhaps first put in practice by Prof. Hebra, but he thinks it is too little known and practiced.

Coffee in Delirium Tremens.—Before the Boston Society for Medical Improvement, as per report in the *Boston Medical and Surgical Journal* for June 28, the subject of delirium tremens was up for discussion. In the expressed opinions of members present, there seemed to be a lack of confidence in the remedial influence of opium. "Dr. Minot asked if any gentleman had tried strong coffee in the treatment of this disease? A former house-pupil at the Hospital, who had seen much of the disease, had great faith in its efficacy, as had also the nurse who took charge of the delirium tremens patients. It is given in the quantity of two quarts in twenty-four hours." * * * "Dr. C. E. Ware had given strong coffee to a patient in the Hospital lately with apparent good effects."

There are but few, if any, diseases in which a greater variety of remedies has been proposed than in delirium tremens. The present must be our occasion for suggesting a modification upon any of the plans of treatment we have ever seen proposed. We believe that *tartar-emetic* and *veratrum viride* possess powers to control the delirium, subdue the peculiar excitement, and thus indirectly dispose to sleep, that are scarcely to be found elsewhere, while in the combined action of *strychnine* and *quinine* we possess an influence admirably adapted to restore lost tone to the nervous system. In the former remedies we have a power to relieve local congestion and vascular excitement, and thus abate the delirium, while with the latter we can give tone and vigor to the cerebro-spinal centres.

Effect of Medicine upon the Teeth.—In our *Summary* for last month we referred to a paper by Dr. Abr. Robertson upon this subject. In

the *Dental Cosmos* for July the subject is continued. It is one of great interest, considering public prejudice; we have space only for a quotation or two. "Having now shown that *medicines*, judiciously exhibited and properly administered, do not, and generally cannot injure the teeth, and that many, if not most, *diseases* do, and of necessity must injure them more or less, I cannot refrain from adding that the *injudicious* exhibition and improper administration of medicines, and more especially the drenching themselves with quack nostrums, to which our people are so strongly prone, as uniformly do, and must, produce ill effects on these organs." * * * "Hence, a safe general rule in relation to quack nostrums is, that any time is an *improper time*, and any quantity is an *improper quantity;* and this is true of all stages of existence, from earliest youth to decrepit age. But medicines, when so administered as to secure the object for which they were given—the restoration of the secretions from an abnormal to their normal condition, the restoration of the body from disease to health—instead of injuring the teeth, protect them from injury."

REVIEWS AND BIBLIOGRAPHY.

The "Journal de la Physiologie."

Though late, we have received the ninth number of this Journal, for January, 1860; containing the usual amount of valuable physiological matter. It is now two years since the *Journal de la Physiologie* was started at Paris—long enough to test the question of its success and permanency. The Journal was commenced on a scale which demanded a considerable success in the way of subscription for its support. Each number contained from two hundred to two hundred and fifty pages; it has been elegantly printed, and apparently no expense has been spared in rendering its illustrations as perfect as possible. The great element of success, however, lay in the contributions it could command from the numerous eminent physiologists now working in Paris. In this respect the journal has surpassed the expectations of all who had formed any opinion on the subject on this side of the Atlantic. Take, for example, the last number, and we find in it original articles by Robin and Magitot, Chauveau, Balbiani, Ollier, Brown-Séquard, Gratiolet, Martin-Magron and Buisson, Rouget, Gubler; another article by Chauveau, and three more by the editor, Brown-Séquard. This constitutes the original department;

and of the names we have mentioned, six, at least, are known where the science of medicine exists. Comparing the original department of the number before us with that of the preceding, and even of the first number, where great efforts were undoubtedly made to obtain contributions, we find that it never has been as rich in material as it is at present. In addition to the articles we have mentioned, we have translations and extracts from other periodicals; proceedings of learned societies, and a summary of the progress of physiology in all nations, in which we may add the United States takes no insignificant position. Conducted and supported in this manner, the Journal is important to every physician, and indispensable to a physiologist.

The first article in the January number, on the development of the dental follicles, etc., by Robin and Magitot, is exceedingly elaborate, occupying fifty pages of the Journal. The subject is treated of too minutely to admit of an analysis at this time. Observations have been made upon the formation and development of the dental follicles in the fœtus of man, some of the mammalia, and some reptiles; and it is found that there is a remarkable uniformity in the development of these organs in the different animals examined; thus enabling observers to draw correct deductions from observation upon the lower animals. This article is to be continued in the following numbers of the journal. The second article, by Chauveau, is on some of the physiological effects of electricity on the animal organism. The next is upon the *rôle* of the generative organs in the multiplication by spontaneous division of certain kinds of infusoria. The reader of the *Buffalo Journal* will remember an article which appeared in the *Journal of Physiology* some months ago on sexual generation in infusoria, which was analyzed for that journal. This article is a continuation of the same subject.

Following this we have an article by Dr. Ollier upon the transplantation of bone. Dr. Ollier has lately been making some exceedingly interesting experiments upon the transplantation of the periosteum, which have already been noticed by us. Transplantation of bone has been occasionally attempted, but with no very satisfactory results. Ollier, however, as shown in this paper, has succeeded in a most marked manner. He divides his experiments into three groups:

1st. "Transplantation of bone taken from a living animal and placed in the midst of the tissues of an animal of the same species."

2nd. "Transplantation of bone taken from an animal dead for a

certain length of time, and placed in the midst of the tissues of an animal of the same species."

3rd. "Transplantation of bone taken from a living animal, and placed in the midst of the tissues of an animal of a different species."

The author's experiments upon these various points have been eminently successful. We give in the author's own words a *résumé* of the most important results of some of his experiments:

"In our experiments we have transplanted bones completely removed from the soft parts, enveloped simply in their periosteum. We have varied them in several respects, in order to appreciate the mechanism of the transplantation, and the different conditions of its success. We have seen, in the first place, that the periosteum played the principal part, and that its presence was indispensable to the success of the operation. It protects the vitality of the bone which it invests, and furnishes matter for its nutrition and growth; it continues, in a word, to perform the functions which usually devolve upon it." * * * * * * *

"We have transplanted bones ten minutes, thirty minutes, an hour and a quarter, after the cessation of the beats of the heart in the animal which furnishes them, and we have succeeded in making them regain their vitality, so that five months after the transplantation we have made an injection penetrate into the medullary canal."

We pass over an article on microcephalus, and the character of the human race, by Gratiolet; the comparative action of strychnine and curara, by Martin-Magron and Buisson; and the morphology of the locomotive apparatus of the vertebrata, by Rouget. We next have experimental researches on the physiology of the medulla oblongata, by Brown-Séquard. In this article the author reasons that the instantaneous stoppage of respiration, and consequent death, which follows upon the destruction of the medulla oblongata, is the consequence of irritation, and not the destruction of the nervous centre. Taking the phenomena which follow destruction of this centre in detail, he argues first, that the arrest of the heart's action is identical with the effect produced by galvanization of the pneumogastrics, by which the action of these nerves is exaggerated, not abolished; and that the medulla oblongata and cervical portion of the cord are often removed without this result. Second, that irritation of various parts of the cord produces that sudden arrest of the respiratory movements which so invariably follow injury of the medulla oblongata, so that the arrest of respiration, which commonly follows the removal of the medulla, may be the result of irritation of the neighboring parts of the

nervous system; and that the movements of respiration have been observed to continue in certain animals after removal of the medulla oblongata. Third, that the "agony" without convulsions, another acknowledged characteristic of death from injury to the medulla oblongata, is seen in cases of death from syncope, in cases where the movements of the heart are arrested by the galvanization of the pneumogastrics, etc., etc. These are but a few of the most important arguments brought forward by the author in support of his views.

We have another short article by Brown-Séquard, showing the independence of the vital properties of the motor nerves. In support of this he adduces the fact that the irritability of a motor nerve which has been entirely detached from its centre, will return after it has been completely exhausted by continued irritation. It is impossible, of course, that this return of its vital property should come from any other part of the nervous system. He also brings forward an experiment in which the vital properties of a nerve were brought back under the influence of blood charged with oxygen, when this nerve was completely isolated from the nervous centres; this was done by injecting the blood into the vessels of a limb of an animal, after the nervous irritability had entirely disappeared. At the end of ten minutes the vital properties of the nerve, as shown by muscular contraction following its irritation, were completely re-established. We have remaining, in the original department, another article by the editor on the influence of the nervous centres on nutrition, and a letter from M. Chauveau on the mechanism of the vascular "*bruits de souffle*," and the action of the auriculo-ventricular valves.

Our notice of the original articles contained in this number has been so extended, that we are compelled to omit the section devoted to translations, selections, etc.

An Address: delivered to the Graduates of the Long Island College Hospital. By Austin Flint, M.D. Brooklyn: 1860.

Any discourse of Dr. Flint, upon whatever topic, is always scholarly, evincing that nice sense of the proprieties of *Belle-lettreism*, which show him to be thoroughly conversant with literature beyond the strict limits of that of his own profession. The rules and spirit which should guide and animate the young practitioner in his endeavor for an honorable distinction in his profession, are very happily drawn and com-

mended in the present discourse. The terms with which he designates the *rear* of the profession have a mild and agreeable spiciness. He entitles this division of the profession as *monumental physicians*. We may thank him for the truth he so neatly enforces in the following paragraph:

"In the medical profession, as in other walks of life, not a few cannot, or will not appreciate the great fact of progress; and, hence, the profession abounds in members who may be distinguished by the title of *monumental physicians*. They represent not the present, but the past! They are living monuments of former stages of the career of science. They do not reflect science as it is, but perpetuate it as it has been. Remaining in a fixed position from the time when they entered the profession, they represent different periods of the past. A few are the living representatives of medicine as it was fifty years ago; some carry us backward a score of years, and others are only removed a single decade. While the progress of knowledge and art is onward, they are stationary, occupying always the same spot, with eyes reverted—'not remembering Lot's wife!' Our monumental brethren are not infrequently among those who decry the present system of medical instruction; finding fault with the schools for not adopting a higher standard of attainment for graduation. This is not the time and place for discussing the responsibility of professors for the admission into the profession of imperfectly educated graduates. But it is not inappropriate, in the present connection, to say that the subject of medical education has reference not to medical students only, but to practitioners of medicine. The want of progressive studies after graduation is, to say the least, as crying an evil as defective preparation for practice. Much as our schools fall short of what they might be made to be, it would be of immense advantage to the profession if practitioners, old as well as young, resorted to them much oftener than they do. As it is, our intelligent students contribute in no small measure to the improvement of the profession by reporting what they see and hear, and thereby inciting and directing the inquiries of those who stand to them in the relation of private teachers. In fact, it is not too much to say that the students and graduates from our schools go forth as missionaries to convert the stationary practitioners to faith in the new revelations of truth which are constantly taking place in the progress of medical knowledge! Let me express the hope, gentlemen, that of those whom I now address, none will ever be ranked among the *monumental physicians*. Begin your professional career with the conviction that constant progress

is the law of this as of other departments of knowledge not springing directly from Divine revelation; and with the determination always to be found among those who represent its actual condition."

The whole of this discourse is noticeable among those on similar topics for the absence of the *Academic strut*, which nauseates the philosophical appetite while overlooking them.

A Monograph upon Aconite: its Therapeutic and Physiological Effects, together with its Uses, and Accurate Statements derived from the Various Sources of Medical Literature. Translated from the German of Dr. Reil, Teacher of Medicine, and Physician at Halle. By HENRY B. MILLARD, A.M., M.D. Prize Essay. New York: William Radde. 1860.

Monographic writings are valuable means of unfolding and portraying any given subject, and tend to improve and enrich our knowledge. Prolixity, which is repugnant to American taste and character, is, however, likely to attend such productions; much unimportant and valueless matter is associated with the good and profitable, yet, on the whole, we consider monographs as holding important places in the literature of medicine, and we wish they were more numerous.

We have just been perusing a monograph on *Aconite* from the pen of one Reil, a German, translated by Dr. Millard, of this city, and published by Radde—all *Hahnemanniacs*.

The treatise is very good in many respects, discussing the subject in its historical, physiological, and therapeutical relations. Upon analysis of the work, we find about one hundred and fifty different authorities quoted, upon whose experiments, opinions, &c., is deduced the *finale* of the book, under a chapter of "Conclusions." When we investigate still further, and find but about twenty-five of these one hundred and fifty authorities are homœopathists, we are a little put to our wits to find out how *homœopathic conclusions* can be drawn from such sources. It is true, conclusions might be deduced negatively, but in this monograph under consideration they are drawn positively; and this only increases our wonderment at the superstructure upon such a foundation, especially as the writer, when speaking of regular medicine, calls it "an almost forgotten old-womanish system."

In the course of our reading, we find the expressions "our remedy," and "our aconite," which would lead one to think aconite was a remedy peculiar to the Hahnemanniacal practice. How such assumption can be made is more than we can understand.

Aconite was introduced into the materia medica centuries ago, long before the diluting, triturating, attenuating, succussing, dynamizating, infinitesimalists started in their career.

Hundreds of pages portraying the individual and collateral experience of learned investigators declare to the world that aconite as a medicine found its birth-place in the manger of regular medicine.

We protest most earnestly against the assumption that aconite is a homœopathic remedy.

Who of all others has given to us the most reliable facts in reference to aconite? Fleming! Of sixteen monographs that have been published on aconite, how many are from infinitesimal pens? *Three!*

Of nine articles that have appeared on the subject in journals, how many are written by homœopathists? *Three!*

Six out of twenty-five authors have written about "our remedy," which makes it homœopathic, of course, and homœopathically it is so. If the number could be attenuated still more, and stood in about the proportion of aconite to its menstruum in its thirtieth dilution, then their right to call it a homœopathic remedy would be indisputable.

Setting aside its homœopathic connections, we think the book worthy the attention of all inquirers after information upon the subject, and would only say that we think its translator has erred in having the work issue from a source which at once stamps the volume with the impress of only infinitesimal value, and thus narrows its circulation.

JOB.

Transactions of the Medical Society of the State of New York for the Year 1860. Pp. 280.

This volume of the Transactions of the Medical Society of this State is not equal to some of its predecessors in bulk, and yet the meeting itself was a very interesting one. Perhaps the chief reason of this discrepancy between the things as done and the things as recorded, is, that much time was taken up by discussions, of which no record appears. It was a notable exception to the rule generally holding true of such societies, that the discussion of by-laws and rules of order took very little of the time of the meetings. The excellent management of the President, Prof. B. F. Barker, kept the Society from that waste of time which follows these common and exciting, but useless discussions. Some of the debates arising from the subject of the various papers were, to our thinking, very well worthy of preservation, and we hope that hereafter measures will be taken to preserve them.

As illustrative of our meaning, we can remind those who were present, that the interesting discussions which followed upon the presentation of the memorial of Queens County Medical Society; of the report of the Committee on Pharmaceutical Preparations; and of the resolutions concerning the pathological specimen of fracture of the neck of the femur, are not given in the proceedings. Yet these were all upon practical subjects of great importance, and the discussions were generally regarded as of great value by those present. But let us not be understood to blame the excellent Secretary of the Society. He has much to do, and he does it well. But what is needed is a short-hand reporter, who shall prepare his account of the proceedings for the Committee on Publication, and let them preserve what is valuable of it. Then would the absent members of the Society be in a manner benefited by the discussions, as well as by the papers read at the meeting.

The number of papers in this volume is thirty-four, and consist of voluntary communications, reports of committees, communications from county societies, and biographical sketches. One of the reports—that of the Committee on Pharmaceutical Preparations—has appeared in our pages. It is, by the way, an interesting fact, that the pharmaceutical report was not adopted, chiefly because it was *too favorable* to those preparations; a view that may surprise the manufacturers.

A careful perusal of the volume will repay any one who receives a copy. A certain number of them, by the way, are at the command of each Assemblyman and Senator.

The New American Cyclopædia: A Popular Dictionary of General Knowledge. Edited by GEORGE RIPLEY and CHARLES A. DANA. Volume X. *Jerusalem—Macferrin.* New York: D. Appleton & Co. 1860. Pp. 788. 8vo.

The depressing effects of the dog-days seem to have no power on the energy and perseverance of the editors and publishers of the Cyclopædia. With almost mathematical regularity, each volume makes its appearance at the promised time, and satisfies the subscribers, by the valuable material it contains, that they have done well in voting the book a prominent place among valued works of reference on their book-shelves. It combines so many good qualities, being a good manual of biography, geography, history, and science in general—a *multum in parvo*—that it is really a library in itself, from which the general student, the specialist, and the man of business may obtain that

information which is required almost daily in their several pursuits. Dry detail is avoided, and the articles present so attractive a form, that the student is tempted to take up a volume in his leisure moments, and read simply as a pastime.

The march of science and knowledge generally is so rapid that Cyclopædias in a few years become antiquated. It is pleasant to think that one of the most useful of these publications has been produced by the American press. Let us hope, when the time shall arrive for the appearance of a successor to this Cyclopædia, it may command a superintendence as liberal and thoroughly Encyclopædic as the publishers have been fortunate enough to obtain for the New American Cyclopædia. L. H. S.

Froriep's Notizen aus dem Gebiete der Natur, und Heilkunde, gesammelt und mitgetheilt von Dr. ROBERT FRORIEP, des rothen Adlerordens vierter Classe Ritter, Königl. Preuss. Geh. Medicinalrathe, A. D. und praktischem Arzte in Weimar, &c., &c. Druck und Verlag, von *Friedrich Mauke*, in Jena.

These *Notes of the Natural and Medical Sciences* are in the thirtieth year of publication. One hundred numbers are issued annually; subscription price, $8.00.

Each number consists of eight beautifully-printed quarto pages, double columns, of the choicest reading matter, arranged under the two separate heads of *Knowledge of Nature*, (Naturkunde,) and *Knowledge of Healing*, (Heilkunde.) Long extracts from new books, introduced by short critical and expository notices, and occasionally, also, important contributions, extracted from the journals, or directly communicated, form the principal articles of each division; while in each, under the sub-heading *Miscellanies*, additional news from the book or periodical press is presented in short, condensed abstracts. Finally, under the heading *Bibliographical News*, a list of the newest publications throughout the world, with full title-page, and generally the price, is given for each of the two departments.

From this indication of the contents, the reader cannot fail to see that every one able to read German who wants to "keep up with the times" in scientific knowledge, be he professional or layman, cannot do better than become a "constant reader" of *Froriep's Notizen*. Every new book that leaves the scientific press is here at once carefully and judiciously gleaned and represented by copious extracts; every important contribution to the scientific journals is condensed and submitted. To the physician, in view of the discoveries and practical im-

provements in medical science; to every man of education, in view of the progress of the science of sciences—that of "nature" in all its interesting and practical bearings—this publication proves the realization of his most sanguine and exacting expectations. Returning editorial courtesy, we can but wish that this monument of scientific erudition and zealous industry, founded by the worthy father, may long receive the well-continued labors of the present editor. L. E.

Archives for Clinical Surgery. By Dr. B. Langenbeck.

Among the latest surgical news from Germany, is that of the establishment by the illustrious Langenbeck, of Berlin, of the first journal devoted exclusively to surgery in Germany, a translation of the name of which heads this notice. It is edited, under Langenbeck, by Prof. Billroth, of Zurich, and Dr. Gurlt, of Berlin, and issued from the well-known press of August Hirschwald. The announcement of these four names is a sufficient guarantee of its worth. Its contents embrace—1, Monographs and essays on surgical subjects, based on clinical observation; 2, Delineation of new or improved methods of operating, carefully illustrated; 3, Description of new or improved surgical instruments, with the requisite illustrations; 4, Statistics of surgical diseases and operations; 5, Examinations in the department of History of Surgery; 6, Topographico-anatomical, pathologico-anatomical, and pathologico-chemical examinations, with direct reference to surgical subjects; 7, Operative surgical experiments on animals; 8, Experiments with external remedial agents; 9, Miscellaneous surgical information. It is also announced in the prospectus, that a yearly report on the "progress of surgical science and art" will be introduced in the *Archives*. Wishing in conclusion, on our part, the most ample success to this new periodical, and the circulation throughout the world to which its merits entitle it, we soon hope to be able to give our readers a taste of the cream of its well-supplied stores. L. E.

EDITORIAL AND MISCELLANEOUS.

— As a large proportion of the mortality of the human race occurs from one form of disease, it is proper that we should give more than ordinary attention to any suggestion in the way of therapeutics or prophylactics which promises success. The first article we give

in this number contains such a suggestion, which is not only advocated by forcible reasoning, but sustained by a number of facts as would lead us to place more reliance upon the efficacy of the treatment than upon many of the plans heretofore proposed. We invite, therefore, our readers to a careful perusal of this paper, and call upon them to further the object which is set forth in the foot-note upon the first page. It will be observed that the paper was read before one of the Sections at the last meeting of the Medical Association, and referred back to the author, to report further investigations at the next meeting. The author invites contributions upon the subject from his professional brethren throughout the country, so that he may be able next year to meet the wishes of the Association.

In order to command attention and merit confidence, all such investigations should be conducted upon the most scrupulous basis of a careful and scientific diagnosis. It is not enough to say that the patient had phthisis; all the rational and physical signs should be minutely noted, so that no carping critic could pick a flaw or question the validity of the diagnosis. The undoubted character of the disease once established, the action of the therapeutic agent should then be noted with the same care and the same exactness.

It is the want of this scientific exactness that occasions the rapid changes, in our estimation, of the value of many of the best articles in the Materia Medica, and the wide discrepancy in the opinions of writers.

Much has yet to be learned about the disease which constitutes the subject of this paper. Indeed, we might say that notwithstanding the numerous researches, but little comparatively is known. Its pathology has been written, but that pathology only proves the destruction of the individual. Spontaneous cures are recorded, but they have only shamed us into the acknowledgment that the ways of Nature are better than our ways, and that our best course is to imitate her. If we cannot begin at the beginning and trace the course of the disease to its fatal termination, we may proceed in the inverse order, and trace the action from its close to its incipiency.

The subject has been one which has especially attracted our attention, and the recent investigations in anatomy, physiology, and pathology have led us to view the disease as one essentially of the lymphatic system, primarily of the tissue, and, as a consequence, subsequently of the lymphatic glands, either locally or generally, extending secondarily, by means of the lymphatic vessels, throughout the whole economy. The views of Virchow, given in his Cellular Pathology; the anatomical researches of Papenheim, and the analogy of syphilis and other

infectious diseases, are confirmatory of the opinion here given. The chlorate of potass would, therefore, seem to be a not inappropriate remedy in the incipient stages of the disease; on the contrary, it appears to us as a most valuable agent for the elimination of the morbific principle, which may be at the very bottom of this evil. We have witnessed most striking effects from analogous remedies, such as the iodide of potassium, chloride of sodium, phosphate of lime, lime-water, &c.

We hope, therefore, that this subject will not receive the ordinary fate of similar calls, but that the plan of treatment proposed will be thoroughly tried by the profession, and the result of these trials faithfully reported to the author of the paper.

— The want of private hospitals where the sick can receive all the benefits of a well-regulated institution, without the unpleasant surroundings of a residence in a public hospital, has long been felt in New York. The recent establishment of a *Maison de Santé*, which we briefly mentioned in our issue for June, has in part supplied this deficiency; and when we say in part, we mean that it constitutes but one of a number which should be organized. Scarcely a city in the Old World that does not possess several hospitals of this character. One or two cities in our own country have adopted them, and New Orleans especially, has always been noted for the number and excellence of its *Maisons de Santé*, which the traveling public have learned to appreciate. Until quite recently there have been no institutions of this kind in this city; here, where the floating population is so large, and the number of sick strangers proportionably great; where so many come for the professional attendance of our metropolitan physicians and surgeons. The inconvenience of hotel life to the invalid is notorious; the small, ill-ventilated rooms, the tardy attendance, and the disproportionate expense, is always a subject of universal remark. A *Maison de Santé* happily meets all these objections in its large, airy rooms, its regulated attendance, and its special appointments for the care of the sick.

The Sanitary Home, under the immediate superintendence of Dr. Henry Schweig, is situated in Second Avenue, near Tenth Street, in one of the most salubrious parts of our city. Its arrangements, when thoroughly known, cannot fail to be appreciated by both the profession and the public: by the former, on account of the assurances they have that they can visit their own patients, knowing that they will receive the best of attendance, and the treatment directed by them rigidly pursued; by the latter, for the reason that they can employ what physician or surgeon they choose, in addition to having the best attendance the city can supply. We say this advisedly, for

we have visited the Sanitary Home, and have surveyed it in every department. As the result of this survey, we can confidently recommend it to the patronage of the profession.

— We call the attention of our readers to the circular of DR. GIBBS, which appears in another part of our journal. If sufficient encouragement be given to justify the undertaking, he proposes to issue *A Year-Book of American Contributions to Medical Science and Literature.* Our readers are familiar with the *Summary* which Dr. Gibbs has, for the last year, prepared for the pages of the MONTHLY, and it would be, therefore, a work of supererogation for us to commend this undertaking to them.

Such a work as is proposed by Dr. Gibbs has long been needed. The medical literature of other countries have their abstracts, retrospects, annuals, and year-books, and we are gratified in knowing that in the pages of the MONTHLY this design originated, which we hope will receive the encouragement it richly deserves.

— With the October number we propose to commence a *Summary of Foreign Medical Literature,* which will give a brief abstract of the leading thoughts of writers in foreign countries. This portion of our journal will be under the supervision of a medical gentleman—DR. L. ELSBERG—who has already gained for himself a reputation in this department. The experience that Dr. Elsberg has had in preparing notes for other journals has eminently fitted him for the work.

Changes in Medical Journals.—The *New York Medical Press,* a weekly journal, whose advent we chronicled eighteen months ago, has no longer an existence. Its last number appeared with the last week of June, and its subscription list was transferred to the publishers of the new weekly, the first number of which appeared on the 7th of July, as was stated in our last issue.

Two years ago there were two medical journals published in Detroit; now there are none. The *Peninsular Medical Journal* and *The Independent* were united at that time, and, until recently, appeared under the title of *The Peninsular and Independent.* A recent issue of this latter announces that it is the last, and that it ceases to exist on account of a lack of pecuniary support. This journal was ably conducted, and we regret that the editors should have failed to receive from the profession of their State a just recognition of their services.

The *Belmont Medical Journal* has also closed its career. This journal, though comprising few pages, showed the true spirit of progress, and we are sorry to hear of its suspension. The reason assigned is a want of support.